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PART I.

ORIGINAL COMMUNICATIONS.

ART. IX.—*On a peculiar Discoloration of the Skin in Females.* By J. T. BANKS, M. D. T. C. D., King's Professor of Physic; Physician to the Whitworth and Hardwicke Hospitals, and to the Richmond Lunatic Asylum, &c. &c.

MY attention having been for some time directed to the consideration of the subject of the discoloration of the skin in females, from the fact of my having under my care, in the Richmond Asylum, a young woman who presents this remarkable phenomenon, and being aware of the paucity of reported cases in which this strange appearance has been observed, it occurs to me that it may be of sufficient interest to place it on record, and, even if nothing more be gained, to add one more observation to those already existing in the annals of medicine. Before giving an account of my own case, I propose to lay before the reader a brief historical sketch of the observations previously published, and, subsequently reviewing all that has been noticed in connexion with the phenomenon, we shall be in a position to show a certain amount of resemblance, at least so far as some associated symptoms are concerned.

The nineteenth volume of this periodical contains a memoir on "A peculiar black Discoloration of the Skin of the

Face," by the learned Editor, from which, although familiar to many, it will be necessary for me to refer in order to give a complete idea of the actual state of knowledge of the subject at present within our reach.

Since the publication of Dr. J. Moore Neligan's paper, Dr. Leroy de Mericourt has contributed five cases which have come under his immediate notice, and he has, moreover, made some highly interesting remarks derived from the observation of this phenomenon.

The first account of discoloration of the face in a female, and by far the most remarkable, is to be found in the Philosophical Transactions for 1709, in a letter from Mr. James Yonge to Dr. Hans Sloane, Secretary of the Royal Society. He says he relates it briefly because the "rarity" is not yet at an end, but, unfortunately, he has not given us the sequel.

"A girl sixteen years old had a few hot pimples rise on her cheeks, which bleeding and a purge or two cured. She continued very well till about a month afterwards, when her face, so far as is usually covered with a vizard mask, suddenly turned black, like that of a *Negro*. This surprizing accident much amaz'd and frightened the girl, especially after some foolish people persuaded her she was bewitched, and never to be cured: by prayers, exorcisms, they endeavoured to relieve and take off the fascination, which, proving ineffectual, the passion and terror of mind increased to a great degree, even to distraction, and then they demanded my assistance."

Mr. Yonge then proceeds to say:—"I directed a lotion for the face, which took off the discolouration, but it returned frequently, but with no regularity, sometimes twice or thrice in twenty-four hours, sometimes five or six times. It appears insensibly to the girl, without pain, sickness, or any symptoms of its approach, except a little warm flushing just before it appears. It easily comes away, and leaves the skin clear and white, but smuts the cloth that wipes it from the face; it feels unchious, and seems like grease and soot or blacking mixt. It hath no tast at all, which is to me rather strange that a fuliginous exudation should be insipid. She never had the menses; is thin, but healthful; the blackness appears nowhere but in the prominent part of her face. There are a thousand eye-witnesses to the truth of this wonder, but I am not able to find or conjecture the cause of it, nor have I ever heard of the like. I shall be glad to know your opinion, and ready to make such further enquiries as you shall please to send, in order to discover the cause of this dark and strange *phenomenon*."

In a second letter Mr. Yonge says:—

"The anomalar blackness of the girl's face, of which I sent the history, is now divided into a few dark, clowdy specks, which appear but seldom, and nothing so livid as formerly."

More than a century elapsed before we find another example on record of this rare affection.

The next notice of this change of colour of the skin to be found in the annals of medical literature occurs in the "Archives Générales de Médecine," 1831, in which Dr. Billard describes the affection under the name of "Cyanopathie Cutanée."

A girl, aged 16, residing at Corzé, presented on the face, the neck, and the superior part of the chest, a beautiful blue "coloration," extended chiefly on the forehead, the alæ nasi, and around the mouth. When the face was rubbed with a white cloth, it became stained with the colouring matter, leaving the skin of its natural hue; her lips were ruddy,—"l'embonpoint assez prononcé;" the pulse regular; and the appetite as in health. The only morbid symptom was a dry cough, but without râle or dulness, and without aberration from the regular type of the heart's action. This girl had menstruated for two years, and there had been no deficiency. At this epoch she perceived that there was a blueness around her eyes, but exposure to the air sufficed for its removal. The phenomenon reappeared when she worked in a confined and hot place; at the end of the year the whole forehead, and indeed the whole face, had become blue, so that the attention of persons who met her was attracted. The catamenia were more abundant, and at the same time there were some sputa of blood, and subsequently hematemesis, but nevertheless, the regularity of the menstrual function was undisturbed. After the use of sulphur and decoction of sarsaparilla, an abundant sweat was observed, and contemporaneously there was a sensible diminution of the urine, and a considerable increase of the blue colour. The forehead, face, neck, chest, and abdomen, presented shades of azure blue, the intensity of which, or their becoming pale, being influenced by the acceleration or retardation of the subcutaneous circulation. The linen of the patient was stained with blue. The sclerotic, the nails, the skin of the scalp, and the concha of the ear retained their usual colour; the buccal mucous membrane was of a pale blue; the tongue was almost always coated; but there was no febrile commotion in the system. Fifteen days later there was complete suppression of urine, which continued for three days; the co-

louring matter became more abundant, and each night there was copious perspiration.

The blue matter having been subjected to the action of reagents, and Dr. Billard, finding that of all those employed, and which were found capable of neutralizing the colour, water rendered slightly alkaline was the most innocent, bicarbonate of soda was administered to the patient. After twelve days the skin of the body and extremities became quite white; on the forehead, around the eyes, and on the nostrils, a blue tinge remained. The general state of the health was improved, still there existed a dry cough, and, from time to time, some hemoptysis, particularly at the approach of the catamenia. Whenever the girl was exposed to much heat, or when she experienced any disagreeable impression, she became more blue than when in a state of tranquillity. Near each menstrual period she felt a sensation of suffocation, palpitations of the heart occurred, and the cough became more severe; she spat up and vomited black blood with effort, as in hematemesis. When the catamenial period had passed, and that she had vomited and expectorated a certain quantity of blood, she became pale, less oppressed in her breathing, and the blue colour almost entirely disappeared. On one occasion when this girl vomited blood, a quantity of the blue matter was deposited at the bottom of the vessel sufficient to stain it, but the colouring material was not collected.

At the time Dr. Billard drew up his observations on this case, the patient was in a most satisfactory condition: the face was but slightly coloured, and the body and limbs did not retain a trace; the forehead and cheeks, however, became blue whenever she was fatigued, agitated, or thwarted. The coloration still became more vivid by heat, its intensity being always in proportion to the increase of transpiration.

As for the circulation, the pulse was small and regular; palpitation occurred when she walked quickly, or was agitated; the urine was abundant; and the dry cough continued. Still there was no loss of flesh, nor was there any diminution of the appetite or strength. Finally, whether from the effect of treatment, or from a simple coincidence, there was evidently a decided improvement.

The next observation upon this strange discoloration of the skin was published by Mr. Teevan, in the twenty-eighth volume of the "Medico-Chirurgical Transactions of London" (1845). The subject of Mr. Teevan's observations was a girl aged 15, who was seized with frequently recurring headach

and pain in the chest, the menstrual secretion being at the same time scanty. All treatment employed with a view to relieve these symptoms was unavailing. About three weeks afterwards a brown spot was observed at the inner angle of the left lower eyelid, which in four or five days became black; at the same time the discoloration of the skin extended, so that at the end of a month the eyelids and the whole forehead, to the hairy scalp, were engaged. At no other part of the body was any abnormal colour observed; the parts affected were exceedingly sensitive, but on being washed with soap and water the pigment was removed, and the skin assumed its normal colour. The excretion of the pigment was more active at night than during the day, so that a part of the skin which had been thoroughly cleansed in the evening was found in the morning coated with a layer of colouring material.

Twice was the patient attacked with erysipelas of the face, without, on the disappearance of the disease, the abnormal hue of the skin undergoing any change. Local applications did not appear to have any influence upon the discoloured skin, powerful astringents, such as decoction of oak-bark with alum, and nitrate of silver in solution (a drachm to an ounce), being ineffectual. About four months from the commencement of the girl's illness she was seized with vomiting, when she ejected two basins-full of dark sour fluid, which, on resting, deposited a quantity of matter resembling soot in odour and appearance. Subsequently the black vomiting was accompanied by the excretion of black urine and black evacuations from the bowels, while the colour disappeared from the skin. After an interval of two days there was a repetition of the same symptoms; the skin became again discoloured, but in a few days the stain began to fade, and finally disappeared. In the next four months there was no relapse, and the patient was perfectly free from headach and pain in the chest.

Subjected to a microscopic examination, the pigment from the forehead was found to consist of short hairs, epithelial cells, granular masses, and fat globules; the reaction, on first examining it, was strongly acid, but afterwards it was not so. It was not soluble in water, and neither caustic alkali nor nitric acid changed the colour. On the application of heat a burnt smell of animal matter was emitted; the ashes were alkaline.

The young girl whose case is reported by Mr. Teevan had been under the care of Dr. Reade of Belfast, who has also furnished details, and who, at the time of Dr. Neligan's publication on this subject, to which I have now to refer, informed

him her health was fully re-established, the disease never having returned.

The next example of this affection which has been brought under notice, occurred in the practice of Dr. Quinan, Physician to the Donnybrook Dispensary, near Dublin, and forms the subject of the important monograph to which I have before alluded.

A girl aged 21, who had been healthy previously, experienced a complete cessation of the catamenia two years before she came under Dr. Quinan's care; soon afterwards erysipelas appeared on the right side of the body, which shortly afterwards disappeared, but returned at each menstrual period. For eleven or twelve months this patch of erysipelatous redness continued to appear and subside at the monthly periods, each eruption of it being preceded by malaise and fever. She suffered from cough and vomiting, her strength at the same time giving way. The redness of the side, however, did not continue to present itself periodically, although the catamenia were still absent. After a fit of retching, blood appeared in the vomited fluid. At the next regular period for the catamenia she vomited about half a pint of reddish-brown matter; and this was repeated for four or five days in succession. The following month the black vomiting again occurred at the ordinary period, and then for the first time a blackish-blue stain was observed at the inner canthus of the eye.

Dr. Quinan supposed, when he first saw it, that it was caused by excessive straining, but on the following morning a large black patch was present under each eye, and the conjunctivæ being unaltered proved that the stain was not caused by the rupture of capillary vessels. The black patches existed under each eye, and assumed a deeper black colour at each menstrual period. The vomiting continued, and her general health was failing.

Such was the history of this patient, as reported by Dr. Quinan, when she was first seen by Dr. Neligan. She was highly excitable, nervous, and hysterical; the dark stains extended, and the upper eyelids were affected; the discoloration reaching the skin of the cheek at one side; the colour was like what would have been produced by Indian ink. On examining the dark patches with a powerful lens, Dr. Neligan found that the stain was not of equally deep colour throughout, but was dotted over the surface of the skin, the dark spots corresponding to the orifices of the sebaceous glands. No attempt was made by him to remove the stain by washing, as the

part was exquisitely tender. On the last occasion of Dr. Neligan's seeing this young woman, her general health was more seriously affected, although the vomiting of dark fluid had somewhat diminished. The discoloration had extended, both cheeks below the malar bones being now engaged, as also the alæ nasi. Around the black stains an exudation of the yellow matter of the sebaceous glands had taken place, presenting the ordinary appearance of "stearrhœa flavescens."

Professor Law observed a case, the history of which, he says, agreed in the minutest particulars with that of Dr. Quinan; the shading of the eyes was identically the same as if it had been effected by Indian ink, the same derangement of the uterine function, and irritability of stomach existed.

In the "Archives Générales de Médecine," Dr. Leroy de Mericourt has published a communication entitled, "Mémoire sur la coloration partielle en noir ou en bleu de la peau chez les femmes."

In addition to referring to the previously reported cases in which this discoloration of the skin was observed, he has brought five more under notice, to which we shall now turn our attention.

Of the first three cases which Dr. Leroy de Mericourt observed, he informs us his information was incomplete; however, he gives a succinct analysis of them. They were all noticed at Brest. The first of which he has any knowledge goes back seven years; the other two, four years at least. The age of the young persons at the time of the appearance of the discoloration of the skin varied from seventeen to twenty years. No accurate knowledge of the state of health previously was obtained, but it appeared to be good. In one case only there existed vertigo, with hysteria and dysmenorrhœa. Twice the dark tinge of the skin appeared on the sudden suppression of the menstrual discharge, caused in one case by cold to the feet, and in the other by mental emotion. The suppression was followed in one case by serious symptoms, viz., syncope, cephalalgia, palpitations of the heart, and a feeling of suffocation. The dark colour showed itself two days after the suppression; at first it was only a blackish-brown spot, which became a beautiful black, invading in succession the lower eyelids, then the upper, and extending beyond the eyebrows. Black spots of variable extent showed themselves on different parts of the body; the tint was more faint in the morning; mental emotion, fatigue, and elevation of temperature rendering them more deep-coloured. At the end of two years the catamenia reappeared; the treatment which was employed having been di-

rected to bringing about this result. The black discoloration persisted even after marriage and many favourable accouchements; however, a notable diminution coincided with the period of lactation. Although at present the menstruation is not perfect, the coloration of the skin has much diminished, and is now reduced to a narrow line on the inferior eyelid, of a deeper hue at the internal angle of the eye.

In another case it was positively proved that the colour was not so deep in the morning, and that it diminished on the eyelids being rubbed with a cloth, which evidently retained a part of the colouring material; but the black efflorescence was reproduced with great rapidity. The same causes which rendered the colour more intense in the former case were equally operative in this. The phenomenon remained without diminution notwithstanding marriage, a fortunate accouchement, and the appearance of perfect health. It persisted in the third case also during the period of pregnancy.

Dr. Leroy de Mericourt next proceeds to give two cases concerning which he was in possession of more accurate details. The first was a young woman, aged 22, lately married, who consulted him for a discoloration of the eyelids, which, however, only existed to a moderate degree compared with what it had been. This young woman was of good constitution; she had brown hair, her complexion good, and her lips rosy. Her state of health antecedently did not present any peculiarity. She menstruated at seventeen, and during the first year the periods were perfectly regular. Four years since, when on a fishing excursion, she went into the water up to her waist, the catamenia being at the time present. This was followed by sudden suppression of the menstrual flux, attended with headach, palpitations of the heart, suffocation, and colic. She was much distressed by cough, and expectoration of black clots of blood. Four days after the suppression she perceived that the lower eyelids had assumed a black hue, which rapidly increased in extent and intensity. The suppression of the catamenia, which lasted four months, was treated by repeated purgatives, leeching, and mustard foot-baths. The menstrual function was re-established, but the discoloration of the skin remained, and was sufficiently remarkable to cause her much distress, preventing her from going abroad, as she excited the curiosity of the persons whom she met.

For a long time she suffered at the monthly epoch, which announced itself, even eight days before its arrival, by pains in the loins, colic, heaviness of the head, and palpitations of the heart. In the interval, all the functions were well performed,

and she never lost flesh. The black colour was less intense in the morning; it was increased by emotion, fatigue, and heat. She had never observed if the towel she used in washing her face was coloured by the black marks.

A homœopathic practitioner was consulted, who recommended that the eyelids should be bathed each morning with absinthe, a liqueur used in the cafés. The colour gradually became less vivid while she employed this topical application, but it must be remembered that the catamenia had returned in abundance.

At the time of the publication of the report of this case, the colour had faded to such an extent as merely to present a "demi-teinte noire," more marked at the inner angle, which had the effect of lending a peculiar brilliancy to the eyes. The young woman's health was good; she merely experienced palpitations of the heart, vertigo, and lassitude, at the approach of the catamenia.

The last case reported by Dr. Leroy de Mericourt was that of a young woman in her twenty-first year, of good constitution, and who had never been ill except once, when, at the age of ten, she had an attack of jaundice. The catamenia had always been regular, and in sufficient quantity, from the age of seventeen. She merely suffered pains resembling colic for one or two hours before the establishment of the flux. Two months before the date of the report, without appreciable cause, and three days after the cessation of the catamenial discharge, which had not been in any respect unusual, she perceived that the under eyelids had assumed a blue colour, which, on the following day, became of a deeper tinge, closely resembling Indian ink. The discoloration was not confined to the lower eyelids, but extended a little to the cheeks; the remainder of the face and the conjunctivæ retained their normal colour. At the moment of the cessation of the catamenia she experienced a sensation of heaviness of the head, followed by inappetency, nausea, &c., &c.

Since the age of fifteen she had palpitations of the heart of short duration, but auscultation and percussion only afforded negative results. There was no unusual sensibility of the eyelids, which were soft to the touch, like satin. The colour is rendered more intense by heat, exertion, emotion, or fatigue; it persists during the night, but is a little diminished in the morning, but it soon acquires the hue, which it retains during the day. Examined by a magnifying glass, the discoloured parts present the following characters:—

The colouring matter is not equally distributed; it is chiefly

accumulated in the areolar grooves; the seat of the most marked depots were the folds of the lids. It had the appearance as if the girl had passed some time in an atmosphere charged with coal-dust, and had washed her face, except the under eyelids.

The colour is due to a multitude of little black grains deposited as an efflorescence on the surface of the skin; on rubbing the abnormally coloured parts with a white cloth it is stained black; but neither by this nor by washing is it possible to restore the integuments to their ordinary colour, the shade is merely rendered less intense, and soon becomes as deep as before. No derangement of the system existing with the exception of a constipated state of the bowels, purgatives were administered, but no influence was exercised by them on the colour. The catamenia appeared at the ordinary epoch, the quantity being neither greater nor less than usual, and no sensible modification of the dark spots was observed. The young woman's health was not alone excellent, but her appetite had increased; she was irritable, however, from being the object of remark to those who met her, in consequence of the singular appearance she presented.

I have now laid before the reader a resumé of the reports of all the cases hitherto recorded, as far as I am aware, in which this strange discoloration of the skin has been noticed, and I shall proceed to give the history of a case at this moment under my own observation, in which the same appearance is manifested, but which, although almost identical in many respects with one at least of the previously recorded examples in which this phenomenon was observed, still differs from all in one important particular.

An unmarried female, aged 23, was admitted into the Richmond Lunatic Asylum on the 17th of September, 1853. Her history may be briefly stated:—She is rather of a strong build, with dark eyes and hair, her face large, and without much expression. She is the daughter of a fisherman, and resided at the Skerries, a village on the coast, a few miles north of Dublin. She was engaged to be married to a young man who was hereditarily predisposed to insanity, and who committed suicide by hanging himself shortly before the time fixed for their marriage. She went to her lover's cottage, and, as she tells the story herself, his mother sent her into the room in which he was suspended; she knew something dreadful had happened, but did not know exactly what it was until, on rushing into the chamber, she struck suddenly and violently against his lifeless body. It appears that the fearful shock thus experienced had

the effect of rapidly overturning her reason ; she became maniacal, and the insanity assumed a suicidal propensity, her chief aim and object being to follow the example set her by her unfortunate lover. Having made repeated attempts at self-destruction, she was placed in the Richmond Penitentiary in the July preceding the date of her admission into the Asylum. Soon after she came into the Asylum, under the care of Dr. Mollan, she was attacked with erysipelas of the face, from which, however, she recovered in a few days. Her state, when first seen by Dr. Mollan, is reported to have been the following. She is restless generally, but at times sits quietly working with the needle, and talking to herself. She is most anxious to be permitted to go to a wood in the neighbourhood of her home, where she might dwell far from the haunts of man, surrounded by leafy trees, and listening to the songs of birds. On more than one occasion she became much excited, and used almost poetic language in giving utterance to her feelings. This, it may be observed, was the more strange, as she had received no education beyond what usually falls to the lot of persons of her rank in life. She evinces great anxiety to impress on all who come near her that she had not been sent to prison for any disgraceful crime, but avoids the subject of the real cause of her confinement. Her nights are disturbed and often almost sleepless, and she has several times endeavoured to cut herself, with a pair of scissors. For the first two months there appeared to be little change in her mental state ; the disposition to self-destruction continued undiminished, frequent attempts having been made to swallow pieces of glass, and to dash her head against the wall. In general she is engaged talking to herself, wrapt up in her own thoughts, and utterly regardless of all that is passing around her. After the lapse of some time she became more composed, generally melancholy, but occasionally almost cheerful. The catamenia have been irregular; the other functions have generally been performed in a healthy manner. For upwards of two years after her admission her state underwent little or no change. She was sometimes for three or four days excited, and then subsided into a quiescent condition.

Dr. Mollan, the senior physician of the Asylum, who is at present at Cannes, has kindly favoured me with a history of this case during the period she was under his care. He informs me that after several months of amenorrhœa she became the subject of periodical hemoptysis, which was evidently vicarious of the menstrual flux. The hemorrhage was generally attended with considerable excitement, often with great distention of the abdomen, and retention of urine. Dr. Mollan

has frequently succeeded in preventing the attacks by leeching the groins, and by the use of the hip-bath two or three days before the expected period; he has, moreover, tried a great variety of treatment, including emmenagogues of all kinds, which have been administered without effect. In a communication which I have lately received from him, he suggests that a trial should be made of galvanism, the current being passed through the ovaries and uterus. From a desire to watch the progress of the case for a short time, uninfluenced by treatment of any kind, I have hitherto abstained from putting into practice the suggestion of my friend and colleague, Dr. Mollan, but, having now satisfied myself, I shall give the galvanism a full and fair trial.

On first seeing this young woman my attention was attracted by a remarkable discoloration around the eyes, but chiefly beneath and extending to the side of the nose: the appearance exactly resembles what one would imagine to have been produced by painting the part with Indian ink, or, perhaps, rather with Prussian blue. The discoloration around the eyes has existed since the period of the cessation of the catamenia, but it becomes of a more vivid hue during the continuance of the periodical hemoptysis. The ordinary duration of the vicarious hemorrhage from the bronchial tubes is from three to four days, and the quantity of blood lost is about what, in the healthy state of the system, might be expected to flow from the uterus. Shortly before the appearance of the hemoptysis she grows restless, and very much agitated; and all the time there is constipation, with a great amount of flatulent distention of the abdomen. Retention of urine also occurs, and it becomes frequently necessary to employ an instrument for its removal. The tongue becomes heavily loaded; and there is repeated and distressing retching, but seldom actual vomiting. During the retching it is observed that the discoloration around the eyes invariably becomes darker.

She speaks wildly and incoherently upon various subjects, but she ever reverts to the all-absorbing and dominant idea which has taken such painful possession of her mind. At these times she has often spoken to me of her lost lover, of his sad end, and of the shock it was to her, and how it suddenly bereft her of reason. She speaks with horror of his having died by his own hand, and of his having thus forfeited salvation; she also mourns over the disgrace of his not being buried in consecrated ground. She blames herself for not having married him, for she thinks he might have been saved. Her lamentations forcibly remind the listener of Bürger's ballad of Leo-

nore. She is at all times most sensitive upon the subject of the blue discoloration of her skin, particularly when she is much excited; but even in the intervals she dislikes being observed, and I had some difficulty in persuading her to permit an artist to take her likeness. Once she said to me that she would rather her eyes were sightless than that she had the blue stains.

In addition to the interest connected with any case presenting the phenomenon under consideration, there is one peculiarity connected with the last, which renders it more than usually so, viz., the fact of the young woman labouring under mental alienation. The existence, also, of vicarious hemorrhage of meteorismus, and of retention of urine, are worthy of especial notice.

Between the case of Dr. Billard and this, there is the closest analogy, and there was one symptom in each of them which did not exist in any of the others, namely, retention of urine.

Dr. Leroy de Mericourt observes, that nine out of the ten cases collected in his paper occurred in persons residing near the sea, and five were in Brest. He asks is this a simple coincidence, or has the maritime atmosphere any influence in the production of this curious disease.

This abnormal coloration of the skin has only been seen in young females, the age varying from sixteen to twenty-two, in the previously reported cases; but in this which I have related, the age was more advanced, the young woman being about twenty-six when the discoloration about the eyes was first noticed.

All the cases were unmarried females when the phenomenon appeared, but in one neither marriage, nor the restoration of the uterine function, nor pregnancy, had any effect on the discoloration of the skin : lactation alone had a favourable influence.

Dr. Leroy de Mericourt writes of one of his cases, "La menstruation s'est retablie mais la coloration noire a persisté assez intense pour lui causer beaucoup de souci." Here there is one circumstance which we may note, but which is exceptional, namely, that the discoloration of the skin may persist where there is no derangement of any kind in the individual's health. This fact throws additional obscurity over a phenomenon in itself sufficiently obscure. The writer just referred to remarks that this phenomenon occurring only in females, and during the period of the activity of the uterus, is a strong reason why we should look for the "point de départ" in a lesion of

its functions, and the coincidence of amenorrhœa or dysmenorrhœa preceding or accompanying it in the greater number of cases, lends confirmation to this view; and the case I have detailed, with that of Dr. Neligan, is calculated to give additional strength to his hypothesis. Agreeing on this point with the Irish observers, our French confrére enters at length into a refutation of the correctness of the names which have been proposed, by which to designate this strange appearance. Not one of the names which have been given appears to be free from objection, and he admits that even his own, "Blepharomelanose," ceases to be applicable in cases where the discoloration extends beyond the eyelids. The same may be said of the name suggested by Professor Law, "Blepharo-melœna;" but, in addition, it leads to the supposition that the colour is caused by an ecchymosis, and this idea cannot be sustained, from the fact that in some instances the colouring matter can be removed by washing; the absence of swelling, and the varying tint under the influence of heat, &c., all tell against its being a blood effusion. The term employed by Billard, "Cyanopathie cutanée," reminds us of cyanosis, a phenomenon totally different.

The designation given this affection by Dr. J. Moore Neligan, "stearrhœa," with the specific epithet, "nigricans," has found little favour with De Mericourt; and, nevertheless, I am inclined to think that the name was not misapplied in the case observed by Dr. Neligan; and, further, it appears to me that in one of his own the term "stearrhœa nigricans" would be a suitable one. Reviewing the cases now before us, I am forced to the conclusion that we are not warranted in assigning to the phenomenon any one of the designations as applicable to all. Doubtless there is no real difference between the cases in which the discoloration is removable by washing, and in those in which, like my own and others, the stain is not thus influenced. The same may be said of the varieties of shade from black to azure blue. With respect to the duration of the discoloration, it has lasted in one case for seven years, and in another it has disappeared in three months.

Utterly unacquainted as we are with the proximate cause of the discoloration of the skin, we are warranted, from the facts before us, in looking upon it as connected in some manner with perverted menstruation. That the skin has in some instances exhibited redness, and in others has poured out blood *periodically*, we have trustworthy proofs.

Mercatus cites a case of menostasis in which redness of one of the cheeks occurred periodically; he also saw a young woman

who presented the extraordinary phenomenon of menstruating by the skin of the fingers, "per minimum manus digitum, et per annularem sinistræ manus, singulis mensibus sanguinem profundi, non copiosum, in religiosâ quâdem fæmina, cui natura menstruam purgationem per uterum denegaverat." Van Swieten, in his chapter headed, "Morbi Virginum," refers to a wonderful case in the following words:—"Mirabile aliud exemplum propriis occulis vidit Lacutus Lusitanus in virgine rustica, triginta et quatuor annorum ad quam a binis medecinæ studiosis adductus fuit, dum patiebatur periodicam sanguinis evacuationem, quæ menstrui fluores vices supplebat: vidit enim, *e pedis sinistri pollicis parte interna pulposa, juxta unguem, emanare rivulum sanguinis meri.*"

Believing that in almost all the cases in which this phenomenon is observed, the functions of the uterus are deranged, we must look upon it as in some respect dependent upon the deviation from the normal standard, and, as Dr. Neligan observes, direct our therapeutic measures to the restoration of the healthy function of that organ; but our experience now tells us that sometimes this does not suffice to remove the discoloration. In such cases we should follow the good advice given by De Mericourt, and which it would be well for the physician to keep steadily before his mind in the treatment of many diseases, "la premiere indication est, à notre avis, *de ne pas nuire* et d'attendre."

Art. X.—*Critical Remarks on the Treatment of Fractures of the Thigh, and the Advocacy of "Butcher's Modification of Liston's Splint."* By Richard G. H. Butcher, Esq., M.R.I.A,, F.R.C.S.I., Examiner in Surgery in the Royal College of Surgeons in Ireland; Surgeon to Mercer's Hospital; and Lecturer on Clinical Surgery, &c., &c.

Having experienced the greatest facility in managing the most complicated fractures of the thigh-bone by the application of the long splint, as modified by myself, I have no hesitation in referring to my former paper, published in this Journal[a], and appending such remarks and illustrative cases, bearing upon the subject, as may lead more forcibly towards securing its adoption. Besides, I am of opinion that the surgeon who has laboured hard, and with devotedness has endeavoured to concentrate his observation on particular practical

[a] Dublin Quarterly Journal, February, 1853.

questions in surgery, jealously ponders over those conclusions arrived at, during the quieter hours afforded to him when released from professional duties. I, for one, believe, if further experience confirms those views, he is bound, as far as the truth with him lies, to sedulously guard, support, and maintain them. But, above all things, it is most imperative, where clinical instruction is daily carried on, and where numbers of pupils are assembled together to study disease at the bedside, as it presents itself, that right views should be inculcated, and that no narrow-mindedness on the part of him appointed to teach should subvert the facts, or divert their legitimate inferences.

An additional reason urges me to a consideration of the subject,—the doctrine set forth recently by the Professor of Clinical Surgery in the University of Edinburgh. It may appear an invidious task thus openly to assail the opinions of one so exalted at home, so enveloped and shrouded by the halo of his own greatness; but if any candid reader pauses for a moment, and considers the flagitious attacks hurled by this learned Professor at the opinions of every school except his own, then, probably, an efficient apology will present itself for departing a little from the prescribed rules of medical etiquette. Thus, for example:—

By the energy and indefatigable labour of one member of the Irish School of Surgery (I allude to the late Dr. Bellingham), we have had the treatment of aneurism by compression established and explained; the inferences which he arrived at being most conclusive, and the facts which he advanced and proved, so forcible, as to attract the notice of the most eminent in the British school of surgery, and receive their highest commendation, their sanction both by teaching and by practice. Indeed, in every class-book of the day, worthy of notice, the subject has been dealt with in the most liberal way, in the spirit of true philosophy. One example will answer my purpose; it is taken from a Scotch author. Thus, Professor Pirrie writes:—"It may now be said that this mode of treatment has received the approval of all leading surgical authorities, with the exception of Mr. Syme"[a]. Mr. Syme's views are thus graciously and modestly expressed:—"So long as it is my sincere persuasion that ligature of the artery is preferable to pressure, for the cure of popliteal aneurism, I shall deem it my duty to pursue this method, *though it may not, perhaps, be the best suited for the lowest capacity of surgical skill.*" And again:—

[a] Pirrie's Surgery, p. 533.

"Let every man act according to his powers; *but let no one who feels it necessary to choose inferior means throw blame upon those who are able to practise a higher exercise of their art*"[a]. It will be admitted, I think, the arrogance of assumption and the insolence of invective are strongly conveyed by the words I have italicised.

But to return to the subject. We have a "Lecture on Fracture of the Thigh," delivered by Mr. Syme, and published in the "Lancet" for February, 1855; and also a paper on "Fracture through the Trochanters of the Thigh-bone," in the "Edinburgh Medical Journal," October, 1857. With the observations on each of these it is now my business to deal, and see how far they are in accordance with the tenets I have laid down. In the paper first referred to, Mr. Syme asks the question:—"Can anything be done by position of the limb (in case of fracture of the thigh-bone), as in the case of the leg?"—and after dwelling on the different sets of muscles accessory to displacement, arrives at the sage conclusion:—"All surgeons who have treated fracture of the thigh have experienced this difficulty, that there is no position that will relax the muscles; and, accordingly, various means have been resorted to for the purpose of counteracting them by extension."

He then proceeds to animadvert upon the several appliances used by surgeons to effect this object, and concludes thus:—"I have no hesitation in saying that, however heavy be the jackstone of Hildanus, or however firmly the patient be tied to the long splint, none of these means can prevent muscular contraction if the tendency to it continue; and if you obstinately persevere in combating muscular contraction with the long splint, you will incur the risk of ulceration of the skin of the perineum, or mortification of the foot." Mr. Syme then asks, "Is the treatment of fracture of the thigh therefore hopeless?" and he proceeds to lay down a method of treatment which, in some instances, as we shall see presently, may be harmless, but in others must be ineffective and ruinous. I perfectly agree with Mr. Syme, that the thigh cannot be placed in any position which will relax all its muscles, but I quite differ with him as to the power which extension exerts in overcoming muscular action. There is, I would say, no other means in this special form of fracture by which the tendency to spastic contraction can be subdued. Surely the expressions of Mr. Syme convey no sensible meaning; he says:—"Do not trust to overcoming muscular contraction by extension, but seek to prevent it by

[a] Edinburgh Monthly Journal, February, 1847, pp. 569, 570.

careful adjustment of the broken bone and equable support of the limb in the proper position." The learned Professor seems altogether to have forgotten the contending force, the muscular contraction; and how the victory 'of careful adjustment of the broken bone' is to be achieved, without first subduing it, can only be answered north of the Tweed. The first instance adduced in this paper to illustrate the position is but a poor example, and one which in reality does not bear at all upon the difficulties to be encountered in the management of fractures of the thigh-bone. The case to which I refer, is that of a boy only "ten years of age, and the fracture situated in the upper third of the thigh," we are told nothing about the amount of shortening,—nothing with regard to the direction of the fracture, or the force which produced it. Again, we should remember that at this early period of life the fracture is usually transverse, that muscular power has little to do with displacement, and, therefore, we can readily understand how that in this instance the ends of the broken bone, when drawn from their abnormal position, probably thrust there by the fracturing force, being supported laterally and around in their normal relationship, mutually afforded support one to the other. Such being my reading of this case, I can easily comprehend how Mr. Syme might "admire the tranquillity of the patient and the completeness of the cure."

I am quite certain that the picture represented here will not convey itself to the mind of the practical surgeon accustomed to treat fractures of the thigh-bone in the adult, powerful man, who probably before the accident was a perfect specimen of muscular development. Oh! no: let the accident take place in this strong man; let the shaft of the thigh-bone be broken in its middle; let the solution of continuity be, as it is found to exist at this time of life, oblique in its character; see the limb shortened from four to five inches, the thigh a distorted, shapeless mass, measuring in its bulky circumference nearly equivalent to its length; see it rigid and motionless, and directed outwards; the integuments discoloured from torn-up and disruptured vessels, and the effusion of their contents; hear the apprehensions of pain, the suffering of the man when spasms seize upon and convulse the muscles, irritated by the sharp spiculæ of the broken fragments,—I would ask any hospital surgeon could he confirm Mr. Syme's report, "that the treatment is extremely simple and uniform." Certainly that laid down by the learned Professor is simple enough:—"Draw out the limb to its full length; then apply at each side of thigh a splint of wood, pasteboard, or leather, extending on the

outer side from the knee to the great trochanter, and on the inner side from the pubis to the inner condyle; it is of little consequence which of these materials be used;" these are to be retained by "looped bandages;" "three or four such looped bandages are applied along the thigh, three being sufficient if the splints be of wood, while an additional one is desirable with leather or pasteboard, on account of their softness at the time of application;" "the spasmodic action of the muscles is continued, in consequence of their being pricked by the sharp ends of the broken bones;" then, as a means of preventing motion of the joints, Desault's splint is laid along the outside of the limb, and retained by a mode which I shall notice presently.

I admit that when the fracture is transverse, probably such mechanical means will be sufficient; but I confidently deny that these two splints can possibly retain the broken fragments in their proper position, however accurately they may have been reduced by extension, and coapted, not only in cases similar to that to which I have adverted, but even in the generality of those occurring in the practice of every surgeon; the very fact of the softness of the material, leather or pasteboard, used in the splints, and indiscriminately recommended, requiring an additional looped bandage for their sustension, proves that the due support cannot be afforded by this method of management, even until Desault's splint is applied along the limb, after a method, as Mr. Syme says, "powerless as a means of overcoming muscular contraction." The faultiness of this entire adjustment of the fracture, from beginning to end, is absolutely proclaimed by Mr. Syme himself, for he says:—"The spasmodic action of the muscles is continued in consequence of their being pricked by the sharp ends of the broken bones;" a condition that will seldom or never maintain, I believe, if the separated fragments be properly coapted. The very method used by this gentleman in retaining Desault's splints in position by means of a sheet, is both clumsy, unsurgical, and defective in affording that equable support to the limb throughout its extent, so salutary in quieting muscular twitchings, and favouring returning circulation. The objection urged against the use of rollers, namely, "though they look very neat when first applied, they soon require re-application," only applies when they are badly put on, and therefore goes for nothing. This I shall presently more forcibly point out, by showing I have had them retain their position for weeks. Mr. Syme continues:—"This apparatus has also the great advantage that it can be ob-

tained extemporaneously; there are few patients, rich or poor, whose houses do not contain a piece of wood that would answer for a long splint, and a sheet or table-cloth to wrap round it." By investigating the matter it will be seen that the poverty which can afford to Mr. Syme those requisites that he demands for adjusting the limb, will be sufficient to offer the same so that by my method a more efficient appliance of them may be made.

A remarkable case is next cited by Mr. Syme, as tending to show the inapplicabilty of the long splint in a certain form of displacement, when the convexity occasioned by the fragments of the thigh-bone is directed backwards. Thus he writes:—"A man was once brought to this hospital with a simple comminuted fracture of the thigh, for which the long splint was applied; the patient, however, obtained no relief from it, but suffered such extreme agony that it made him even get out of bed and drag the splint after him. I confess he met with but little sympathy, being supposed to be affected with delirium tremens. After a while, swelling of the limb came on, followed by ulceration and protrusion of bone in the ham, and I was obliged to amputate the thigh. On examination of the parts, we found that the fragments, instead of forming, as usual, a convexity forwards, projected backwards; and one fragment, as sharp as a needle, had penetrated the popliteal nerve, and obliterated the vein, so that the extreme pain and the swelling of the limb were both accounted for." Now, I ask candidly, is not this case a perfect exposition of the imperfect application of the splint? It is quite clear, if the proper extension and counter-extension, not only essential for the adjustment of the broken fragments, but likewise for maintaining them in position, had been kept up, so unfortunate an occurrence as "protrusion of the bone in the ham" could not have taken place. It is also not impossible to conceive that by due tension upon the part, the "fragment that penetrated the popliteal nerve" may have been set free; but be this as it may, I have no hesitation in stating, if the two forces had been brought properly to bear upon the limb, that the unusual direction of the fragments backwards would have offered no impediment to their reduction; while the long splint, if applied after the principles I have laid down, would have assuredly maintained them in their proper axis, and probably have averted the calamitous result—amputation.

I have determinedly stated that the direction of the fragments backwards in "comminuted fracture of the thigh" is amenable to treatment, by the modification of the long splint

which I employ; and, in proof, shall extract from my case-book the following particulars of an instance of fracture of the thigh-bone, as unpromising, and offering as numerous and serious impediments in its treatment, as any ordinarily met, besides presenting the displacement adverted to.

Case.—A young man, aged 28, of low stature, but of powerful frame, was returning from a fair in the north of Ireland, having purchased several valuable horses. He was having them conveyed by train to Dublin, and superintended their management. As the train passed along, some sparks from the engine blew into one of the horse-boxes, and fired the straw; the animals began to kick and plunge furiously; so, in order to release them from suffering, and to save his property, the young man jumped into the box, and had just succeeded in putting out the fire, when one of the animals lashed at him with both hinder feet, striking his right thigh in front, and smashing it to pieces. In this condition he lay huddled up in the corner of the box, suffering extreme agony, afraid to move, and thus he continued several hours until the arrival of the train in Dublin. He had no way of communicating his sufferings, and had no mode of escape. I was called to see him late at night, and with my former pupil, Dr. Daniell, proceeded to the place. I shall transcribe verbatim the entry in the case-book. The patient was screaming with torture as he lay extended upon the bed. Many anxious friends were endeavouring to restrain him in the recumbent position. Having slit up the boot and trousers, and removed all his clothes, the following distortion was present:—The anterior configuration of the limb was such as I had but seldom before witnessed; it was a large, bulky, rounded mass; there was an indentation in the front, forming a well-marked concavity, while behind, and somewhat towards the outside about the centre, was the apex of a very remarkable convexity; the leg and foot were everted and motionless, until at times spasms would convulse the thigh, and drag upwards this hanging, almost lifeless portion of the member; these spasms were quick in production, oft repeated, and distortive. On seizing the thigh above and below the fracture, the leg being controlled by my very intelligent assistant, the solution of continuity was evident by the free motion permitted laterally about the centre of the femur; antero-posterior movement was more limited, and the cause was readily accounted for by the direction of the upper and lower fragments of the broken bone being thrust backwards, and from their having passed each other. Besides these two continuous portions of the bone, numerous fragments were

broken up in the vicinity, evidencing the violence by which the mischief was occasioned; in other words, there was incontrovertible evidence as to how wide an extent the bone had been shattered; and this, in some measure, accounted for the extreme shortening of the limb. I have said in some measure, because, independent altogether of the comminution of the bone, the upper and lower fragments were thrust widely beyond each other, and hence the great difficulty in reduction.

On measurement, the thigh was found to be nearly five inches shorter than the sound one; to add to the deformity, blood was extravasated in large quantity all through the site of fracture, and along the upper third of the thigh: besides this, in many points small bullæ had made their appearance, owing to the intense heat to which the limb was subjected by the burning straw having set on fire the right leg of his trousers and drawers.

In this case, as well as in many others which I have met with, the reduction of the fracture is a long, tedious process, to be obtained only by gentleness: the difficulties to one inexperienced in such matters appear at first insurmountable, and, indeed, often the necessity for prolonged, continuous traction might shake the confidence of the operator, were it not steadied and based on sound principles. In the instance before us, the man being very muscular, and in the prime of life, the difficulties met and contended against were extreme: it required four strong men, relieving each other, to keep up a gentle, steady, even traction upon the limb, and that continued for over twenty minutes before the bones were reduced, and the limb restored to its proper axis. Such being accomplished, I applied the long splint, as modified by myself, subjected to each and every precaution that I have insisted upon in my published directions. In accordance with the rules which I have laid down, after the adjustment the patient was removed to a bed prepared as I have also described in the management of these cases. Immediately after, the man was unbounded in expressions of gratitude; he was perfectly released from the torturing apprehension of the frequently recurring spasms; he rested in peace, free from all pain. Large opiates and warm wine were freely given throughout the night, as the nervous system had sustained great shock and alarming derangement. The patient was sedulously watched day after day, lest the mechanical appliances should create pain or excoriation, but no such thing. Carefully and efficiently applied at first, it did not require alteration or removal until the 14th of October,

six days after its application, and, but for the irritable state of the skin occasioned by the burns, there was no necessity for disturbing the splint.

The report on the 25th is:—Patient doing well: no requirement for interfering with the splint; the full length of the limb steadily maintained.

On the 28th readjusted the splint. Limb full length; the carded wool has been salutary in healing all abraded parts; ecchymosis rapidly disappearing. The splint was not again changed until the 11th of November, when the broken fragments were found in admirable position.

On the 30th of November the splint was removed altogether, the union of the broken bone having been perfected. The patient, intolerant of restraint, would not submit to longer restriction or confinement to bed. The length of the limb was most perfect, and preserved almost in its entirety. The most careful measurement could not detect more than a quarter to half an inch shortening, though the comminution of the bone at the site of fracture, as already dwelt upon, was considerable.

In the several adjustments of the splint, and in the institution of the measurements, I was always assisted by my pupil, Dr. Daniell, who for many years was accustomed to observe and assist me in the management of similar embarrassing cases under my care in hospital. On the 1st of December the man moved about on crutches, and immediately after proceeded to England.

I shall now make a few observations relative to the applicability of the long splint in the treatment of fractures of the thigh bone in early life and in childhood. In the paper to which I have referred, written in 1853, I made no mention of cases treated at this tender age, not because they did not come under my observation, but simply from the fact of their comparatively easy adjustment. To illustrate the treatment of fractures of the thigh bone I selected, from amongst many, the most intractable cases in adult life, implicating different parts of the member, as recognised by anatomical nomenclature. I will not deny the fact that I have met with some troublesome cases of fracture of the thigh in childhood, with considerable obliquity in the line of solution of continuity in the bone, and rest confident that such could not be treated satisfactorily, or restored to their normal length, except by the application of the long splint, according to the method which I have laid down. I shall record a few instances to illustrate this statement; I shall narrate them as entered in my case-book.

Case I.—William Carey, aged eleven years, admitted into

Mercer's Hospital, under my care, June 1, 1854. He was thrown down by a car, and in the fall his right thigh was broken in the centre. The solution between the fragments very oblique, and shortening to an amount of two inches and a half; limb very convex externally and anteriorly; by gentle and continued traction restored to full length, and the long splint applied according to the precautions which I have written about.

June 3rd. All spasms checked; no pain or inconvenience from the constraint of the mechanical appliance.

13th. Readjusted splint; limb preserves its full length.

26th. Reapplied the splint; limb as at last report.

July 15th. Removed the splint; union perfect; not the slightest excoriation at the parts particularly subjected to pressure, and not the least shortening.

July 18th. Dismissed cured.

Case II.—William Lord, aged twelve years, admitted into Mercer's Hospital October 14, 1854. He was holding on behind a cab, and in his struggles to retain his place his left thigh got in between the spokes of the wheel and the bone was broken below its centre. The amount of shortening here was nearly three inches; the fracture was exceedingly oblique, and there was no comminution of the bone; so sharp was the upper fragment that it lay close to the integument in front, and it was difficult to conceive how it was not thrust through the soft parts, rendering the injury compound. I was summoned at once to see the child, so extreme was the shock, even endangering life. After resuscitating him by stimulants, &c., I at once restored the fragments to their natural relationship, and applied my own splint.

October 16th. Reaction was fully established, and the limb in admirable position. The child complained of no pain.

19th. No pain; no necessity for the splint being disturbed.

25th. Report same as last.

November 6th. This day readjusted the splint for the first time since the child's admission; thigh bone preserves its full length.

15th. No pain or annoyance from the constraint adopted.

18th. Removed the splint altogether; union perfect; no shortening.

24th. Child dismissed cured.

I have intentionally selected those cases from amongst many others, occurring at a period before Mr. Syme's lecture appeared, and shall confine myself to the two to illustrate the particular point, the facility with which even oblique fracture of the thigh bone can be rectified by permanent extension when

judiciously employed. I shall also take from my case-books a few instances dated even at an earlier time, to show with what facility fracture of the thigh bone may be managed in the child, when the solution of continuity is in the transverse direction that most frequently met with during this period of life.

Case III.—Patrick Carroll, aged ten years, admitted into Mercer's Hospital, under my care, September 28, 1853. While playing with his school-fellows he was thrown down, and his right thigh fractured a little below its centre; the deformity was not very great, but the leg and knee rotated outwards. On examination, the bone was detected broken transversely; the parts were easily reduced, and the long splint at once applied, affording the greatest relief and immunity from pain.

The splint was not readjusted until the 9th of October, and the fracture was in excellent position.

October 27th. Limb remains free from pain; no necessity to disturb the apparatus.

31st. Removed the splint; union of fracture quite firm; length of limb perfectly preserved; reapplied the splint; to remain for a few days longer, as a matter of safety; and began gentle motion of the knee and ankle.

November 10th. Dismissed cured, with the motions of the knee and ankle entirely restored.

Case IV.—Daniel Mooney, aged fourteen years, admitted into Mercer's Hospital, under my care, February 20, 1851. The boy was running along a plank which was considerably elevated, he slipped off and fell to the ground, a height of about four feet; when taken up, it was ascertained, even by the by-standers, that his right thigh was broken. He was at once carried to the hospital, and the right thigh pronounced fractured at its centre. The separation in the bone was transverse, and there was but little deformity; spasms were annoying, but were instantly checked by the application of the long splint, which I adjusted at once. On the 24th the splint was reapplied, when the limb was found to preserve its full length. The splint was not changed again until the 17th March; and afterwards, on the 22nd of the month, at this date, the femur was quite firm, and the thigh presented its full length. On the 27th the splint was laid aside altogether, and the motions of the knee and ankle commenced, in a few days after the child was sent home cured.

I consider it quite unnecessary to enumerate any additional cases in proof of the assertion which I have advanced; and with regard to the "displacement" commented on by Mr. Syme, "the tilting of the upper fragment forwards by the action of

the psoas and iliacus," it but rarely occurs, but more especially at this early period of life; and even when it does, it is quickly and certainly subdued by gently applied pressure and continued permanent extension. In my paper on Fractures of the Thigh Bone, to which I have referred, I have dwelt at some length upon this special deformity when occurring in the adult, and pointed out by demonstration both its occasion and the method of its control. I can quite understand how Mr. Syme may have had success in the management of fracture of the thigh in children, even in the application of the long splint as imperfectly adjusted according to his directions. The solution of the question is afforded by the positions which I have dwelt upon: the comparative frequency of the fracture being transverse; the feeble vigour of the muscular system as a distorting force, and the facility with which it can be overcome; and the practical lesson known to all surgeons,—how that if deformity should occur at this tender age, even though the limb be considerably shortened, yet after some time all trace of the irregularities will be removed, so that the ignorant surgeon might begin to congratulate himself upon the superiority of his mode of treatment, while in reality nature has secured the successful issue. This is certainly a happy consummation for Mr. Syme to be aware of, and to dwell upon, in the management of oblique fractures of the thigh bone in childhood: as to transverse fractures, the simplest means will rectify them; but, unhappily, nature's efforts will prove abortive in rescuing the surgeon from annoyance, disappointment, and disgrace, and the patient from deformity, when oblique fracture of the thigh has been sustained at a more advanced period of life.

The example adduced by Mr. Syme, in his paper published in the "Edinburgh Medical Journal" for October, 1857, is, I consider, equally defective in substantiating his views. The case is one of "fracture through the trochanters of the thigh bone." The line of separation was similar to that which the late Mr. Guthrie had the merit of showing might cause inversion of the limb. Mr. Syme is, I conceive, deeply in error when he writes:—"In cases of eversion it is impossible to discriminate with certainty, by external examination, between these two conditions (alluding to fractures within and without the capsule); but when the toes are turned inwards, there cannot be any doubt as to the fracture admitting of complete recovery." From this sentence it would appear that Mr. Syme is satisfied with Mr. Guthrie's explanation as applicable to every case where inversion takes place, but inversion of the limb may take place when the fracture is intra-capsular, as re-

corded by Dupuytren, Cruveilhier, Hanby, Elk, Smith, Malgaigne, and others. No doubt, all these great authorities differ in some particulars as to an explanation of the matter, but the fact remains, and sweeps away Mr. Syme's axiom in reference to inversion, *per se*, as comparatively of little value in influencing the prognosis.

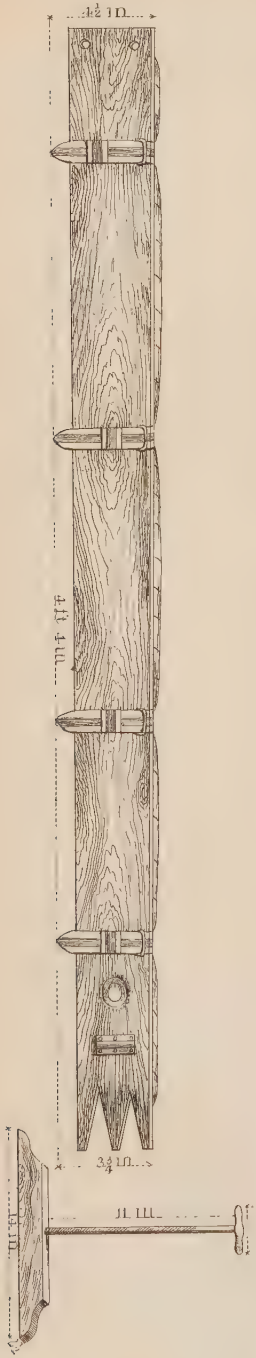
Having diagnosed the accident, Mr. Syme continues:—"In regard to the means of treatment, there does not appear to be any room for choice, since the one unquestionably most comfortable to the patient, and most conducive to the end in view, is the long splint of Desault; it was employed in this case with such success, that the patient left the hospital without there being almost any appreciable difference between the limbs by external examination." I am quite sure that in this instance there could have been no shortening of any amount from beginning to end, no more than what was present when the patient left the hospital; if there was, from the manner in which the splint was put on, and from the intentions which guided the surgeon, it must have remained permanent to the end. I am certain that there could have been no amount of shortening after the accident; and if any proof were required, it is afforded by the reiterated statement of Mr. Syme, "that the long splint is not required to maintain extension, and that the good effects produced by it depend entirely upon the prevention of motion in the limb." The "same details" about the adjustment of the fracture that I have already dwelt upon are again urged in this recent production.

Every surgeon must have met with cases of transverse fracture, or nearly so, through the trochanter, when very little shortening indicated the nature of the injury; such a case, for instance, as the following might be satisfactorily managed by laying the long splint to the outside of the limb, and binding it there according to Mr. Syme's directions:—

Case.—Margaret Cavanagh, aged 65, admitted to Mercer's Hospital, under my care, February 23, 1855. She was struck violently on the right hip by the wheel of a car passing rapidly by; she was thrown to the ground, and unable to rise without assistance. She was soon brought to the hospital, and on examining carefully the limb, it was found to be broken transversely through the great trochanter. The limb lay very nearly in its normal position upon its posterior surface, and neither rotated inwards nor outwards; the amount of shortening was not more than half an inch; there was fulness in front over the seat of injury, and fulness behind the trochanter from extravasation of blood when the force was applied;

the least motion induced great pain, and in lifting the limb gently from the bed, the solution of continuity through the trochanter was marked; the slightest inversion or eversion of the shaft of the thigh-bone elicited crepitus. I applied my own splint, but there was here no difficulty to contend against in the adjustment of the fracture. The splint was not disturbed until the 16th of March, and there was neither abrasion nor pain complained of; on the 25th readjusted, and union far advanced. On the 6th of February I removed the splint altogether, the union being quite solid. On the 15th she could elevate and rotate the limb inwards and outwards without pain; and on the 20th she left the hospital able to move about with the assistance of a stick. Other cases of fracture present themselves, however, in this vicinity, and will demand the most strenuous exertions on the part of the surgeon, and the most perfect extension, to secure the limb in its full length after union. The following case will illustrate the point:—

CASE.—Arundell Ward, aged 36, admitted to Mercer's Hospital on the morning of June 8, 1855. She was drunk at the time, and was violently thrust off the foot-path by a man, and she was pitched with great violence upon her right hip. She was brought to hospital on a car, and after being placed in bed and stripped, the following were the appearances: limb shortened to one inch and three-quarters, and everted; the trochanter was not so prominent as upon the opposite side; and it was nearer the crest of the ilium; the transverse width of the thigh above was considerably increased. On fixing the pelvis evenly, and steadying it on the bed, and extension being made, crepitus was elicited audibly, and repeated upon rotation of the limb inwards; great pain on rotatory movement; the trochanter did not travel in its full arc of a circle, not more than half its usual extent; neither did it rotate, as it were, upon a pivot; the line of separation, in fact, could be felt traversing the great trochanter obliquely. The limb was put up at once with Liston's long splint, and I had great difficulty in overcoming the spasms, and restoring the bone to its full length. Liston's splint was put on because I had not one of my own splints disengaged. I ordered a full opiate, to be repeated at intervals. On the following morning, the 9th, she complained a good deal of the pressure round the ankle from the extension, and more particularly of the heel, when it rested upon the bed. Having procured one of my own splints, I applied it in place of Liston's, and with the greatest comfort to the patient; a few turns of the screw



Lith Dublin.

raised the heel from the bed, and delivered it from pressure. I did not find it necessary to readjust the splint until the 14th of July; of course the perineal lac was changed several times, strictly in accordance with the directions which I have given. According to the same rules, the splint was reapplied, the limb retaining its perfect length, and on the 27th it was taken off altogether, the union being solidly secured. On the 4th of August, the patient in moving about could lean well upon the foot. She continued steadily to improve; and on the 26th she was dismissed, able to walk well, and without any difference in length between the limbs.

The annexed plate is taken from a patient of mine, with the long splint, as modified by me, applied. For its description, mode of adjustment, &c., I must refer to the original communication, "On Fractures of the Thigh Bone," already adverted to.

It is not imperative, I conceive, to adduce any further proof to show the necessity for gentle, steady, and continued extension being kept up during the treatment of fracture of the thigh bone occurring with any degree of obliquity, and in all muscular subjects.

I have the greatest confidence in referring the reader to my paper on "Fractures of the Thigh Bone;" every fact therein stated has, I am happy to say, been confirmed by well-tried and additional experience; and if there is any one thing that I am more certain about than another, it is the vast superiority which the method I advocate possesses over every other in the management of these cases. I shall introduce here the letter of one who has watched cautiously the practice which I inculcate: it is from the senior surgeon of our hospital, one of the most practical surgeons of the day, and the best lecturer on surgery that I ever heard.

"My dear Butcher,—As you are about to publish additional observations on fractures, I wish to bear my testimony to the efficiency of Liston's splint, as modified and improved by you in the treatment of these injuries in the thigh. I have seen many cases in Mercer's Hospital, under your care, of fracture of this bone, in strong, muscular persons, with great deformity and shortening, where the limb was preserved in its normal length during the entire period required for union, and in some instances without a necessity of readjusting the retentive apparatus more than once during the treatment. Everything depends on the skilful appliance of the splint; the

inexperienced in the handling of fractures may consider this a facile proceeding, but such is not the case, as you and I well know.

"Always faithfully yours,

"William Tagert.

"54, *Camden-street*,

"29*th January*, 1858."

Mr. Syme sums up his opinions in the "Edinburgh Monthly Journal," in the three following aphorisms:—

"1. That the great requisites for treating fractures successfully are coaptation and immobility.

"2. That extension, or a struggle between the two opposing forces of muscular contraction and a mechanical power, is not consistent with either of these conditions.

"3. That, therefore, extension should be abandoned in the treatment of fractures."

I trust I have shown that the *first* is reasonable and correct; that the *second* is absurd, because by perfection in the extension alone can "coaptation and immobility" be secured; that the *third*, "extension should be abandoned in the treatment of fractures," is ridiculous, because essential to the fulfilment of aphorism the first.

To those who have not read my original paper on "Fractures of the Thigh," I would merely say, I think they will find every objection that has been urged by writers against the use of the long splint fairly argued, combated, and answered, as also the great advantages which it possesses, as modified by me, over every other mechanical appliance recommended in the treatment of fracture of the thigh-bone, no matter in what part it may be injured; moreover, numerous examples are introduced to prove its efficiency.

There is one point more I would advert to—the improved mode of detaching the transverse piece of wood, as described by me in the "Dublin Quarterly Journal" for February, 1856, where the advantages accruing from such a change will be found strictly described.

Art. XI.— *Observations on some Points of Treatment in Uterine Hemorrhage, &c.* By Samuel Bell Labatt, formerly Master of the Rotundo Lying-in Hospital. Edited by his Son, Hamilton Labatt, F.R.C.S.I.

[The following observations I found amongst the papers of my late father. The extensive practical experience of the author has induced me to think they may be acceptable for publication in the Dublin Quarterly Journal of Medical Science.— H. L.]

A lady came to Dublin in the spring of last year, and engaged my attendance for her confinement, which she expected would very shortly take place. She told me she had been six times pregnant; that she had carried four children to the full time; a fifth to the eighth month; and on the sixth occasion she miscarried at the third month. She always had excessive discharges, at each succeeding confinement the hemorrhage appearing more and more alarming; and upon the last occasion, two years before, the losses and consequent faintings were so urgent and long-continued, that her attendants were for several hours in a state of the greatest apprehension for her safety; she, however, struggled through, but had a slow recovery.

I found her weak, nervous, annoyed with a troublesome cough, and constantly harassed with unfavourable anticipations as to the result of her approaching illness. Her friends told me she never recovered her strength perfectly since her first confinement, though she did not at that time suffer much loss.

Having endeavoured as much as possible to remove her doubts and fears, I recommended her to take one grain of sulphate of zinc, and two of myrrh, three times a day, to use gentle exercise in the open air daily, and to attend to her bowels.

Her labour coming on at the expected time, I was summoned, and found her walking through her chamber, with pains frequent and sharp, and greatly depressed in spirits. I kept her out of bed until the first stage of labour was completed, and the child's head on the perineum; she was then put to bed, and the pains becoming strong, the head was soon expelled. Taking the necessary precaution to insure a slow progress of the child through the passages, I applied my hand on the abdomen, and followed the contracting uterus with a firm pressure. The child being born, I immediately grasped the uterus, which was well contracted, in my hand, and firmly maintained it in that

condition, and having given a draught which I had in readiness, of twenty-five drops of Batley's sedative liquor, forty drops of volatile aromatic spirit, and ten drachms of camphor mixture, I turned the patient on her back, and remained at the bedside for five hours, with the uterus steadily, and without one moment's intermission, secured in the manner I have described. The placenta was thrown off in an hour after the child, and no hemorrhage or fainting followed.

Between five and six hours after delivery, I proceeded carefully to substitute for the pressure made with my hand a towel, folded like a pad, placed over the uterus, and secured by a roller passed tightly round the loins, and then, having had the bed made comfortable, and given the patient some gruel, she fell asleep, and awoke in two hours, much refreshed, and no untoward symptom followed. She had a good supply of milk for her baby on the third day, was in the drawing-room on the fifteenth, and abroad in her carriage early in the fourth week.

I think myself justified in attributing much of the success in the treatment of this case to the great caution used in securing the contraction of the uterus, following up, as I did, the practice formerly recommended by Mr. Charles White of Manchester, and more recently by the late Dr. Clarke of this city.

In my lectures, twenty years ago, I thought it sufficient to recommend the pupils in the Lying-in Hospital to apply, immediately after the birth of the child, a pad over the uterus, secured by a well-adjusted binder passed round the hips; and this practice may suffice in ordinary cases; but when we have reason to apprehend hemorrhage after delivery, I feel assured, by ample experience, that uterine contraction and consequent avoidance of hemorrhage will be better effected and secured by the hand, in the manner I have mentioned, than by any roller, however judiciously contrived; and the pressure should be steadily kept up for four, five, or six hours, according to circumstances. Having often observed the salutary effects of the adoption of this practice at one time, and the bad effects of the neglect of it at another, in the same patient, I can with confidence recommend it to the attention of my junior brethren, who will please to observe the more important points in the management of the case I have detailed. First, I kept the patient out of bed, as advised by Dr. Denman, till the child was on the point of being born, being of opinion, with Dr. Denman, that the erect position is favourable to uterine contraction. Secondly, I allowed the child to be entirely expelled

by the action of the uterus, and even opposed some resistance to its progress through the passages, and immediately after administered a cordial anodyne draught, which I have for many years been in the habit of doing in such cases, and often with great advantage, always combining the opiate with a cordial. Thirdly, I firmly held the uterine tumour in my grasp for several hours, and then applied a pad and roller. It was the invariable practice of all the old practitioners in this city to give a glass of burnt brandy with nutmeg, immediately after delivery, and I think such a cordial will often be found beneficial, by promoting uterine contraction, and thereby favouring the separation and expulsion of the placenta, and I cannot say that I have ever known it to produce injurious consequences.

An interesting circumstance connected with the practice in question occurred to me many years ago. On the evening of the memorable 23rd of May, 1798, a tradesman from Island-street called upon me to request I would immediately visit his wife, who had been seized with a violent flooding after delivery. I accompanied him to his residence, but before my arrival the hemorrhage had ceased, and the poor woman appeared much better than I expected. The midwife I found standing over the patient, and engaged in pressing with all her strength on the abdomen, to prevent, as she said, "the womb from rising up and suffocating the woman." On her discontinuing the pressure by my desire, which she did very reluctantly, the womb immediately enlarged as much as it had been before delivery, and the patient soon became very faint, though there was no external hemorrhage, and begged to be raised up; the midwife then turned to me, and with an air of consequence said, "You see, sir, I was right, and if we don't take care, the poor woman will surely be suffocated," and at once resumed her post, and continued to press down the womb. A great quantity of coagula was discharged, and the patient having taken some spirits in cold water again revived, and the old midwife, whose practice was better than her theory, remained for several hours at her post, in the full assurance that by keeping the womb from "wandering about," she would preserve the woman from all danger.

I cannot too strongly impress upon the junior practitioner the necessity of closely watching his hemorrhagic patient for some hours after all alarming symptoms have subsided, having known several cases of sudden and unexpected sinking and death long after all danger was supposed to have ceased.

Many years ago, a lady of high rank was delivered in

Sackville-street by an eminent and experienced practitioner. Hemorrhage came on soon after delivery, with fainting fits, which, however, were relieved by the ordinary means; two hours after the symptoms had entirely ceased, the lady appeared to be so well that he thought he might with safety leave her. He was suddenly recalled in an hour after, and on entering the room he found her *in articulo mortis*. There did not appear to have been any return of hemorrhage, nor could any cause be assigned for the unexpected event.

About the same time I was called to see the wife of a respectable shopkeeper in Henry-street, who had been seized with profuse flooding after delivery. I found her alarmingly weak, extremities cold, and pulse at times imperceptible. By the free use of cordials, cold air, and other means, she revived, and, the hemorrhage having ceased, the patient was considered to be out of danger. Two hours after, there being in the meantime no return of discharge or weakness, the lady's attendant thought, with me, that we might safely retire, but an hour had scarcely elapsed when we were hastily summoned to see our patient breathe her last.

A healthy young woman was delivered in the Lying-in Hospital (while I was Assistant to the late Dr. Evory, in 1800), of her first child, after an easy labour; the placenta was thrown off in the usual time, but there soon came on a most alarming hemorrhage, followed by faintings. By the introduction of the hand and the aid of cordials, the woman was soon relieved from her perilous situation, and appeared to go on well for some hours, when she suddenly, and without any return of hemorrhage, became weak, fainted, and expired. A post-mortem examination threw no light on this case. The uterus, Dr. Evory thought, was rather more relaxed than usual, and it contained a few coagula.

Some years ago, a lady in Temple-street, the mother of several children, had profuse flooding, attended with fainting-fits, after delivery. She had the assistance of three eminent men in consultation; they succeeded in checking the discharge, and the lady appeared to go on favourably for some hours, they took their leave, but in no very long time they were suddenly recalled, and found their patient expiring.

I have reason to think that death is sometimes occasioned by nursetenders incautiously changing and shifting patients too soon after delivery. An intelligent medical friend told me, some time ago, that a patient he had attended lost her life in this way. The lady, who had been rather weak, was raised by the nursetender to a sitting posture to take some gruel,

when she suddenly fell back and expired. A similar accident had nearly occurred lately from similar mismanagement to a patient of mine.

Nursetenders, therefore, should be frequently admonished and apprised of the danger of disturbing, or raising from the recumbent posture, recently delivered patients, especially after hemorrhage.

Although I do not at present mean to treat of the general management of uterine hemorrhage, I think it well to offer a few remarks on the use of opium, which has of late been largely used by some practitioners. I have read accounts of cases of flooding, in which seven or eight hundred drops of tincture of opium were given within six or eight hours, and, as alleged, with the best effects. The opinion which I had long held respecting the nature of the proximate cause of uterine hemorrhage in childbed, and the effects I had often witnessed of large doses of opium on uterine action, led me to suppose that it was not likely, in such large doses, to prove beneficial in the complaint in question. However, the extraordinary success attributed to the practice by some respectable practitioners led me to give it a fair trial, and the result has been a conviction on my mind that opium ought not to be given too freely in floodings attended with great weakness. I have generally observed that, when administered in very large doses, it increases the weakness, disorders the stomach, and tends to suspend the healthy uterine contraction, and prolong rather than check the discharge. It is good practice to give a moderate dose of black drop, or Batley's sedative liquor combined with burned brandy, volatile aromatic spirit, or Hoffman's liquor, immediately after the birth of the child, when we have reason to apprehend flooding, and the same may be repeated if necessary.

It is generally supposed that if, after the expulsion of the placenta, the uterus be felt well contracted, small, round, and firm over the pubis, there is no reason to apprehend hemorrhage; this may be true to a certain extent; nevertheless, if due care be not taken to maintain this state of salutary contraction by the means already advised, the uterus may relax, and hemorrhage ensue; therefore, the cautious attendant will patiently continue his preventive measures till all immediate danger of hemorrhage shall have subsided, and then apply his pad and roller.

I would here beg to offer a suggestion, for the guidance of the young practitioner in his attendance during the anxious and critical period of childbirth. I would strongly urge him

to remain with his patient as much as he possibly can, from the commencement of labour to its termination; his presence will be acceptable to the friends of the patient, comfortable and cheering to herself, and she will have the benefit of seasonable advice and assistance on any of those emergencies which every now and then unexpectedly occur in the progress of labour. But this is not all: the presence of the confidential medical attendant, and his humane and kind deportment, will inspire the sufferer with confidence and hope, and thus, by the well-known influence of mental impressions on the action of the uterus, will tend to lead the case to a speedy and happy issue. I do not pretend to say that such is the invariable result, for I have known cases to go on slowly where the practitioner remained for hours on the spot, and where, during his short absence, pains suddenly increased, and accomplished the delivery before his return; but I have seen more than sufficient to justify me in asserting, that if accoucheurs would remain more at the bedside of the patient than is the practice of the present day, and sometimes pretend to assist, with the intention, as Dr. Denman remarks, of giving confidence to the patient, or composing her mind, the duration of labour and sufferings of the patient would often be abridged; and I dare to say that the success of Dr. Hamilton, who assures us that "no patient under his charge for the last thirty-five years has been above twenty-four hours in labour, and excepting in cases of disproportion, none so long," was more owing to this circumstance than to any direct manual aid he may have afforded.

Several years ago, I was requested by an eminent practitioner to wait with a lady, upon whom he had been some hours in attendance, in labour of her first child, while he paid an urgent visit in a distant part of the city. The labour, he said, was proceeding favourably, the pains strong and regular. On my going into her apartment, the patient manifested discontent and want of confidence, which I could not remove. The labour slackened, the pains became weaker, irregular, and during two hours that I remained, no progress was made, and on his return the doctor was disappointed and surprised at the change that had taken place. Ineffectual pains continued to harass the patient for several hours, when circumstances occurred to require manual aid, and a dead child was extracted with the forceps. My friend, who was an intelligent and observing man, candidly expressed great regret at having left the patient, who would, he thought, have been safely delivered without assistance, had he remained with her.

Upon another occasion I was hastily called to the assist-

ance of a lady in active labour of her fourth child,—her usual attendant, in whom she had great confidence, being engaged elsewhere; the nursetender said the pains were strong, and labour far advanced, and that the lady, who always had quick labours, would very shortly be well. However, on the lady being told of the absence of her own friend, and seeing a stranger at her bedside, she became much dispirited, the labour flagged, and made no advance for a considerable time, till her attendant made his appearance, when pains soon returned, and she was speedily delivered. Cases like these, showing how much the uterine functions are influenced by the state of the mind, are by no means unfrequent, and should be borne in mind by practitioners in midwifery.

I wish to record the result of my experience upon another subject which has divided the opinions of physiologists and physicians: I allude to the alleged recurrence of the menstrual discharge during pregnancy. Females have often assured me of their having had their regular monthly discharges during the whole or a portion of the period of pregnancy. On closely investigating these cases, however, I invariably discovered, that the characteristic marks of natural and regular menstruation were for the most part absent. The discharge not returning with the usual regularity as to time, nor continuing uninterruptedly the usual number of days, the fluid discharged also differing in quantity and quality from the true menstrual secretion; and it is further worthy of remark, that in several cases which came under my immediate observation, females who asserted that they menstruated while pregnant acknowledged that they had not on such occasions certain feelings which accompanied their usual monthly periods. I may, therefore, safely affirm, that I have not, during a pretty extensive practice of forty years, seen a case of regular menstruation during pregnancy, the instances which occurred in my practice of discharges of blood from the vagina during utero-gestation being obviously cases of pure hemorrhage, so that I feel strongly disposed to lay it down as a practical axiom, that pregnant women never menstruate.

I may here be permitted to offer a few remarks on a very important subject which has lately been discussed by Dr. Hamilton of Edinburgh, and Dr. Collins of this city. I allude to the practice of artificially dilating the os uteri, with the view of shortening the duration of labour, and I do so chiefly from a wish to in-

form those gentlemen who formerly attended my lectures, that I still entertain the same opinion of the practice in question which I then endeavoured to inculcate. In the early part of my professional life I happened to be well acquainted with an accoucheur of extraordinary address and dexterity in the operative part of midwifery, particularly in the use of the "lever". This gentleman was in the habit constantly of applying artificial dilatation in the way recommended by Dr. Hamilton; and having in a few cases witnessed the success of his mode of assisting, I became a convert to the practice, which I adopted for some years, till I was satisfied, by the result of further observation and experience, that although cases did occasionally occur in which labour seemed to have been protracted by want of disposition in the os uteri to dilate, and in which, on using a moderate force with one or two fingers during a pain, I soon effected complete dilatation, and, the head being cleared from the os uteri, pains, excited possibly by the gentle dilating efforts, became more frequent and efficient, and delivery accomplished sooner, perhaps, than otherwise would have been the case; yet that such artificial interference not only frequently failed in hastening delivery, but every now and then evidently appeared to retard the progress of labour, and lay the foundation of local ailment and slow recovery. And having, on my appointment to the Mastership of the Lying-in Hospital in 1814, well considered the subject, I felt that I should not recommend the mode of assisting in question as a rule of practice to the pupils of the Hospital. I have not that very great apprehension with which Dr. Hamilton seems to be impressed, of the injurious consequences likely to arise from the retardation of the *first* stage of labour. Retardation of the *second* stage is more likely to affect the safety of mother and child; and it is not, I am satisfied, by using artificial dilatation with the view of expediting the first, that we are most likely to provide for a speedy and favourable termination of the second stage—often quite the contrary: over-anxiety to accomplish the dilatation, and finish the first stage of labour, has often rendered instrumental delivery necessary in cases which, if left to nature's efforts, would have been finished without the interposition of art, as indeed the experienced gentleman just alluded to has often admitted to me; but he would then add that, "by clearing the head from the uterus, we could apply the 'lever' whenever we pleased, and thus save the patient much pain, and ourselves much anxiety and watching." Sure I am that the woman is less likely to suffer injury from a few

hours' protraction of labour, if she be otherwise well treated, than from the practice recommended by Dr. Hamilton; and I do think Dr. Collins entitled to the thanks of the junior accoucheur for cautioning him against a practice which is fraught with danger.

Art. XII.—*Illustrations of the Opiate Treatment of Rheumatic Fever.* By R. W. O'Donovan, M. R. C. S. L., Belturbet.

There are few diseases, not alone from the intensity of the patients' sufferings, but also from the serious injury to organs whose healthy action becomes a vital question, which call for the sympathy of the profession so much as rheumatic fever. Nor is there a disease which at various times has been the subject of more diversified practice. Theories widely differing have introduced remedies of the most opposite tendencies, each for a time the fashion or the favourite, to fall into disrepute as experience proved its inability to cope with its formidable antagonist.

The treatment of acute rheumatism with opium, either alone or in combination with calomel, is not a new idea. I find, in the very able work of Dr. Fuller, that Dr. Cazenave of Pau was the first to recommend the exhibition of large and repeated doses of opium for the cure of acute rheumatism; but it was from Dr. Corrigan's Lectures I learned to prescribe the medicine in this disease; and, after some years' practice, I give the result of my experience, believing that cases well authenticated, carefully kept, and candidly offered to the profession, are the best means of testing the validity of any particular line of treatment. These circumstances prevented my giving from *memory* the history of others, which, from inadvertence or pressure on my time, I did not take notes of; but I may state that all were treated with opium, and terminated satisfactorily.

Case I.—The first case of rheumatic fever I attended was Denis Fitzpatrick, on the 6th of August, 1841, a small farmer, resident six miles from me in a dry and elevated district. He states he has been ill for thirteen weeks with acute rheumatism: his knees and shoulders have alternately been swelled, red, and painful; the intensity of the pain varying, sometimes exeeedingly severe, then abating, and again returning with great intensity. He has now entirely lost rest and appetite, is emaciated and hectic; has profuse sweats, and is unable to turn in the bed without assistance; this day the left shoulder is painful, red, and swelled; the right knee is much swollen,

not red, but painful in one spot; flesh soddened; pulse extremely rapid; tongue pale and moist; has not slept for some nights. Ordered twelve grains of opium in twelve pills, one to be taken every second hour.

8th. Pain much relieved; slept last night; bowels confined. To have an oil draught; continue his pills; to take a little fresh meat.

12th. Pains have entirely left him since yesterday; sleeps well; appetite improved; perspirations gone; pulse 80; to take two grains of sulphate of quina three times a day.

16th. Is quite convalescent, gets up every day, and is rapidly gaining flesh and strength.

26th. Owing to Fitzpatrick's going out at night to assist in extinguishing a fire in a neighbouring house, he has relapsed. The joints are again affected. He was ordered to take a grain of opium every second hour.

28th. The pains have again yielded to the opium; ordered two grains of sulphate of quina three times a day; the joints to be rubbed with ammonia liniment, in which sulphur is suspended.

September 17th. Steadily improving, pulse 76; appetite good; sleeps well; joints free from swelling; bowels regular; to continue the quina.

Case II.—I was called to visit Mrs. R., a married woman, aged 50, February 27, 1844: she had been ill eight days. The shoulders, wrists, and knees are affected, the pain sometimes ceasing when the joints become enlarged; her agony is very great; she has not slept for the last seven nights, except a little last night; this morning the left shoulder is particularly painful; pulse 126, soft and full; tongue coated, moist, and clean at the edge; bowels moved; perspires profusely; to take a grain of opium every second hour, and the shoulders to be stuped with a decoction of camomile flowers and opium; the wrists to be frequently rubbed with the following liniment,—sweet oil, two ounces; tincture of opium, an ounce and a half; solution of ammonia, four drachms.

28th. Enjoyed a good night, though she did not sleep soundly, yet was free from pain; has taken seven grains of opium during the last twenty-four hours; has no headach; bowels have not been moved for thirty-six hours, but as the abdomen is soft, and she feels no inconvenience, I do not wish to give her any aperient. She feels the perspirations very debilitating, and a papular eruption has appeared over the body; has no appetite, and makes use of a little panada only; pulse 100. To take one ounce of the following mixture,—sulphate

of quina, twelve grains; aromatic sulphuric acid, one drachm; water, six ounces.

March 1st. Spent a restless night, but not from pain; had a large fetid motion; skin has a decided yellow tinge; skin hot, covered with profuse sweat; as the pains returned in the evening, she was ordered two grains of opium every third hour, and sinapisms to be applied to the shoulders.

2nd. Took six grains of opium during the night; pains gone; had some hours' quiet sleep; to continue the opium pill every fourth hour. 10 o'clock P. M. Spent a good day entirely free from pain; had a full motion; no sweats, skin cooler; within the last twenty-four hours she took twelve grains of opium; to continue the pills.

3rd. Omitted the pills, and spent an indifferent night; sweated profusely, and suffered from increased soreness in the knees and ankles; countenance muddy; to have a blue pill and draught, and to recommence the opium pills.

4th. Is considerably better, pulse 102; took the pills regularly, and slept well; perspirations less; tongue cleaning; is recovering the command of her limbs, and can turn in the bed without pain; took ten grains of opium in the last twenty-four hours; to have one grain of opium three times a day, and recommence the quina mixture.

5th. She felt one or two shocks of severe pain during the night, which, however, did not continue long; feels better; pulse 96; pills to be continued.

6th. Is improving, pulse 86; urine scanty, depositing lithates; to continue the pills, and get a diuretic mixture.

8th. Is much better; urine copious and clear; bowels regular; countenance recovering its healthy aspect; is able to sit up in bed; appetite improving.

13th. Is quite convalescent; has had no return of pain for several days, and has not taken any opium since the 8th; joints quite free from swelling or tenderness; she is rapidly gaining strength, and walks about the room.

Case III.—Miles Hughes, a robust, healthy young man, aged 26, after being exposed to cold and wet, was attacked with rheumatic fever: saw him on March 2, the sixth day of his illness; is suffering from acute pain in his elbows and wrists, knees and ankles, which are also red and swollen; skin hot, but covered with clammy sweat; tongue coated in the centre, red and dry at the edges; bowels confined for two days; has not slept for the last five days or nights; pulse 86, soft. To have an oil draught, and take two grains of opium every third hour.

3rd. Bowels freely moved; pain, swelling, and redness abated; feels much improved in every respect; had three hours of refreshing sleep; to continue the opium.

4th. Steadily improves.

6th. With the exception of a little stiffness in the joints, he is convalescent. To use a stimulating liniment, and take two grains of sulphate of quina three times a day.

10th. Is quite well; did not relapse.

Case IV.—September 30, 1848. Visited Mr. E., a young married man, a farmer, who had just recovered from a bad attack of syphilis, with phagedenic ulceration, rarely met with in country practice, in which I attended him. His treatment consisted of mercury and opium, sarsaparilla, and nitric acid, and iodide of potassium. On the morning of the 28th he went out very early through his farm, and got his feet and legs wet and chilled, felt unwell in the evening, and next morning awoke with, to use his own expression, "pains in every joint of his body." Took a warm bath and some aperient medicine before I saw him. Evening. Impossible almost to say whether he has continued or rheumatic fever: complains of pains everywhere, but particularly in his knee-joints; skin burning; the perspirations profuse; pulse quick and strong; face flushed; violent headach; tongue coated. To get calomel and antimonial powder, and a draught of senna in the morning.

October 1st. All the large joints are affected, enlarged, red, and painful; profuse sour perspirations over the entire body; urine scanty; tongue coated in the centre, edges a fiery red; bowels have been freely moved. To get two grains of opium every second hour.

2nd. Took fourteen grains of opium since yesterday's visit; all his symptoms are relieved except the perspirations; these are quite as severe, and distress him very much; he spent an easy night, without sound sleep; towards morning was attacked with severe diarrhœa; the motions literally run from him; even a drink, he says, is no sooner swallowed than he felt it running through him. To omit the opium, and get an astringent mixture.

3rd. Diarrhœa relieved; but pains have returned as severe as before; all the other symptoms remain as on previous visit. Ordered two grains of opium to be taken every third hour; the joints to be fomented with an opiate stupe, and the astringent mixture to be continued if necessary.

4th. Is better; pain relieved, but tumefaction of the joints very considerable; perspirations less; urine more copious. Continue pills.

5th. Pains in the joints nearly gone, but he complains of severe pain along the tibia, and is more irritable than yesterday. To take one grain of calomel, with one of opium, every fourth hour; an anodyne liniment to be rubbed along the tibia.

6th. Is a little easier. Continue pills.

7th. Slight mercurial fetor; pains better; slept last night; bowels regular. To take one pill night and morning.

8th. Evidence of mercurialization more marked; is better. To take an opium and calomel pill at night; ordered compound decoction of sarsaparilla, with iodide of potassium.

This young man was kept under the combined influence of mercury and iodide of potassium for five or six weeks, and ultimately recovered well, though the progress of the case was very tedious. He had, however, no return of the rheumatism.

Case V.—October 26, 1851. Visited James Kemmis, a healthy young man, aged 21, ill eight days. After a night's exposure to cold and wet, was attacked with general febrile symptoms; on the third day he complained of fixed pain in his knees and shoulders, which were also sore to the touch, and swollen; has profuse perspirations, unaccompanied by relief; starts in his sleep. Got a mercurial purgative and Dover's powder before I saw him. This day the right wrist, left shoulder, and both knees are affected, with general pain in the back; tongue furred, red, and clean at the edge; skin hot and moist; pulse 86. Ordered twelve grains of opium in twelve pills, one to be taken every third hour.

28th. Took the pills regularly, and feels so much better, he thinks he could get up. To take a grain of opium at night, and the joints to be rubbed with a stimulating liniment containing sulphur.

30th. Is convalescent, but looks pale; to take two grains of sulphate of quina three times a day.

Case VI.—Donohoe, aged 19, a blacksmith, intemperate: was attacked with rheumatic fever in March, 1854; saw him for the first time the seventh day of his illness.

March 6th. Complains of his left knee, which is very much swelled, with a bright red spot over the external condyle; is exceedingly irritable; has had no rest; confined bowels, and scanty urine; complains also of pain in the back of the neck, and between the shoulders. To have six leeches applied to the knee; thirty grains of compound powder of jalap, and after the action of the powder to take two grains of opium every second hour.

7th. Spent an easy night; took ten grains of opium since

last visit; left knee much relieved; pain and redness gone right knee attacked, but not so severe; perspiration over face and chest, but not very profuse. Continue the pills.

8th. Is better this morning; no fresh invasion of the disease; joints less swollen; sleeps quietly; had a smart bowel attack, without any apparent cause. To get chalk mixture with tincture of catechu; is rather more stupid than I have usually observed. To omit the opium, and have the abdomen rubbed with oil of turpentine, and the knees with liniment of ammonia.

9th. Diarrhœa relieved; slept quietly; urine abundant; swelling decreasing; observed some tremor of the hands. Ordered porter, meat, and quina.

11th. Free from rheumatism, but has some tendency to delirium tremens. To be allowed a glass of punch, and continue the meat.

13th. Convalescent.

Case VII.—Mrs. M., an extremely fat, unwieldy woman, was admitted with acute rheumatism, 27th March, 1855, the joints principally affected on the wrists and fingers of both extremities; very little constitutional disturbance. To take one grain of opium every third hour; five grains of calomel and senna draught in three hours after.

March 28th. Bowels moved; discharges offensive; urine scanty, muddy, and high-coloured; perspires profusely; pain and swelling as yesterday; only took two pills: she is rather a positive sort of person, and cannot fancy a pill so small can possibly be of service; very irritable and restless.

29th. Took the pills regularly; pain greatly alleviated; had some sleep, and is more tranquil and manageable; bowels confined. To repeat calomel and senna draught; pills to be continued; hands and wrists to be rubbed with a stimulating liniment, and swathed in flannel, as she complains of the cold air increasing the pain.

30th. Pain entirely gone; the hands continue swelled and puffy, and urine scanty; the feet and ankles œdematous. To take three grains of iodide of potassium in a diuretic mixture three times a day.

April 2nd. No return of rheumatic pain; urine increased in quantity, with less sediment: discharges from the bowels less fetid; swelling of wrists and hands persistent; to continue her mixture, and have mild iodine ointment rubbed into the affected joints.

8th. Is improving, but slowly. Continue her medicine, and allow light nourishing diet.

14th. Convalescent.

To relieve the pain, which is often agonizing, should be the first effort of professional skill, and, beyond all question, opium stands prominent as the natural ally of the physician, not timidly, but boldly administered. In reference solely to the amount of pain to be overcome, I think it is very difficult to define positively the dose to be given, but, at the same time, I feel the best practice is to commence with a full dose, as two grains every second or third hour, and to continue this quantity until the pain is relieved, and the patient gets some sleep, or at least feels easy and composed; nor should the practitioner be afraid that this large dose would affect the head, as, so far from such an evil being likely to result, immediate benefit will be found to follow its use, even in cases where delirium is present. In Mrs. R.'s case the wandering and delirium ceased on the administration of the second pill (two grains), though it was very violent, and accompanied by considerable heat of scalp.

That the opium treatment will shorten the duration of the disease will scarcely admit of question. The case of Fitzpatrick strongly supports this view. Ill for thirteen weeks, and treated on different principles, bad as he could well be, it was almost unfair to test a new remedy in such a case; but what was the result?—In two days there was a marked improvement; in six days he was convalescent, though I did not deem it prudent to lose sight of him; and when he relapsed we find the disease again yielding in two days, and perfect convalescence established in five.

Hughes was attended on the sixth day of his illness, and was convalescent in four days.

Mr. E.'s case, though complicated with syphilitic taint, yielded to the opium treatment in a very short time; eight days only elapsed before all the rheumatic symptoms were relieved; he then became a case of secondary syphilis, extremely tedious.

Kemmis was convalescent in four days, and had no relapse.

Donoghoe's illness was subdued in seven days.

Mrs. M., who laboured under fatty degeneration of the entire system, recovered in eighteen days; she, however, was free from rheumatism several days before reported convalescent. And Mrs. R., though a long time ill, suffered from a series of relapses rather than one continuous attack, and I much doubt whether she would have recovered sooner under any other treatment.

I have observed that patients treated with opium have not

a tendency to be attacked with chronic rheumatism. I have often inquired from all my patients, with the exception of Mr. E., who emigrated some months after his recovery, whether they were ever attacked with chronic pain, and in no instance has any of them suffered from this wearying and harassing complaint.

The next symptom which calls for investigation, and which is most distressing to the patient, is the peculiar and characteristic sweat. On this point I must differ from Dr. Fuller, who considers the "perspiration beneficial;" and states:—"The materies morbi is in great measure got rid of, and the natural cure of the disease effected, by means of these profuse, sour-smelling perspirations." And further, that "if it [the perspiration] be checked for a time,—if the outlet by which such enormous acid secretions are making their escape be closed, it will require no very accurate observation to mark the *consequent* increase in the severity of the symptoms." I am disposed, notwithstanding so high an authority, to consider this peculiar perspiration a constituent symptom, commencing (as a general rule), continuing, and declining, *pari passu* with the pain, swelling, and other characteristic symptoms. If it be beneficial—strange that the patient *never feels relieved by it;* on the contrary, it is complained of as distressing and debilitating; and if it be the "natural cure," clear judgment would advise its increase by every means at the physician's command, by warm and vapour baths, sudorifics, &c., yet to this treatment Dr. Fuller strongly objects, and I think judiciously. The perspiration of acute rheumatism will meet its parallel in some cases of continued fever, where a sweat, often profuse, not critical, and certainly not tending to relieve febrile action, shows itself almost at the onset of the disease, and often tests the skill and patience of the medical attendant. These cases are also characterized by scanty and high-coloured urine. Fitzpatrick's case was accompanied by profuse perspirations and soddened skin. A case is also mentioned by Dr. Fuller, where, on the thirteenth day, the patient "did not sleep soundly; still continues to *perspire profusely;* and the skin is becoming sodden." Ordered an acid mixture with sulphate of quina and sulphate of magnesia, and a grain of opium at night. Reported on the fourteenth as having "slept more soundly, and is in every respect better to-day; pains much easier; *not so much perspiration.*" It appears to me that the perspiration yielded to the acidulated quina mixture, and that it was ordered with this special intention. Strange it should be so if the perspiration was performing a natural cure.

I have always found this excessive sweating yield to opium, whose *modus operandi* appears easily explained. Opium, it is well known, in large doses acts as a direct sedative on the arterial system, tending to equalize the circulation, and thus relieve the capillary system of blood-vessels; lowering the pulse, as well as tranquillizing the nervous system; permitting the different viscera to resume their healthy action. So that, in fact, as we find the morbid action of the skin decrease, the normal action of the kidneys is found to increase.

I shall conclude these observations by the simple statement, that I have never met a case of cardiac complication: and whether the treatment by opium has a tendency to save the patient from this serious addition to his sufferings, or not, I shall leave to future observers to determine.

Art. XIII.—*Formation of a New Nose.* Case II. By John Hamilton, Surgeon to the Richmond Hospital.

The extreme rarity of Taliacotian operations in Ireland leads me to submit to the readers of this Journal another case in which a lost nose was successfully restored.

Richard Stark, aged 20, a labourer, was admitted into the Richmond Hospital. Half of his nose has been destroyed by lupus; the tip and a greater part of the alæ, and all the cartilaginous septum, are gone; the remains of the alæ have turned in and united so as to close the opening of the nose, all but a small aperture that would admit a large pea. There is a good deal of white cicatrix about the upper lip, drawing up the lip slightly, and everting the prolabium, making the lip thick, prominent, and ugly. A small, clean, round ulcer exists on the lower lip, next the right angle of the mouth, and from the sides of the remains of the nose on each side, a red patch of scurfy papular eruption extends to the cheeks for two or three inches.

The state of the nose, besides the great disfigurement, is further distressing from interfering with respiration, as he cannot breathe through it unless he raises the upper rim of the small opening with his finger-nail. He is otherwise healthy. The disease began eight years ago with a little pimple on the tip of the nose, with redness all around it. Nitrate of silver, he says, was applied, and various internal medicines administered in the Cork and Limerick Infirmaries, but it continued for five years slowly eating away till three years since, when it stopped.

If a probe is passed into the opening of the nose, it can be felt through the integument which lies over the nasal aperture.

The small ulcer at the angle of the mouth was the only evidence of the existence of the lupoid disposition, but quite enough to lead me to put him under a course of hydriodate of potash and sarsaparilla before doing any operation to remedy the deformity. At the end of six weeks it was quite healed; and on the 30th of September, 1857, I proceeded to operate.

I had marked, the day before, with a piece of nitrate of silver, the line of the incision of the ace-of-clubs-shaped flap of the forehead, and also that round the end of the stump of the old nose; this last line extended about a half an inch from what would have been the border of the nose.

In consequence of the septum having been destroyed in front, but the integument at the end of the nose not completely so, this last, as I have already described, had fallen in, and left so small an opening, that he could scarcely breathe through it. The first part of the operation was the removal of this fallen-in and adherent portion of the skin. I did this by passing a probe-bistoury into the small opening, and slitting horizontally on each side, then cutting obliquely upwards from each end to the centre. I thus removed a triangular portion, and made a large opening into the nose. The incision all round the end of the nose (which I had previously marked out) was then made, and the integument within this line dissected off, so as to leave a broad raw margin round the end of the nose. This, and the previous part of the operation were attended with a very free flow of blood, and to prevent its running down into the pharynx by the posterior nostrils, as he lay on his back, a piece of sponge was stuffed into the nostril.

The flap was then cut from the forehead and turned down, and as the blood flowed freely from the exposed surface, and still more from the edges of the wound, pieces of fuzzy lint and sponge dipped in cold water were applied. When all bleeding had ceased, the flap was brought down over the nose, and by twisting it round, the raw surface and its edges were adjusted to the incisions round the end of the nasal stump. They fitted most accurately, and were secured by six points of suture: the incision at the lower end of the forehead, just above where the root of the forehead flap had been turned down, was approximated by a needle and a twisted suture, and a point of suture above this. The remaining bare surface on the forehead was dressed with lint, well smeared with simple ointment. The new nose was seen to flap to and fro when he breathed.

The mere operative proceedings only occupied a few minutes, but it required a long time before the bleeding had entirely ceased, altogether about one hour and a quarter. During the operation the patient was under the influence of chloroform. After the first few minutes of inhaling, he gulped two or three times, as if going to be sick, but it went off. But after all was over he vomited up some blackish matter, evidently blood which had been swallowed, and became altered in the stomach.

Fourth day. He was rather feverish yesterday, and complained of the soreness of his forehead, but the nose looked well; the flap of a natural colour and warmth. I did not remove the dressings over the nose till to-day, when I was happy to find that complete union had taken place on each side, everywhere. I thought it best, however, not to touch the sutures; but I removed the needle at the wound between the eye-brows, and found this part also united; I left the ligature above this, and did not touch the dressing on the forehead, but put a little water-dressing over it. There was some plugging of lint in the nose, which I considered would come away looser in a day or two; and I was anxious to disturb as little as possible the newly united parts.

Sixth day. I took out the ligatures.

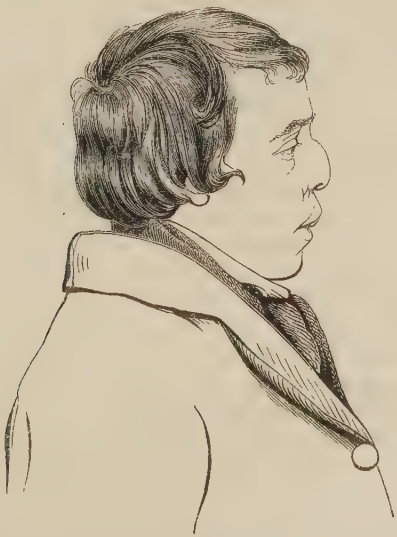
Ninth day. Everything in a most desirable condition. The line of union on each side perfectly consolidated; the new-nose flap of good colour and temperature. The wound in the forehead granulating and filling up rapidly. General health good. Eight weeks after the operation the nose was getting quite natural in appearance, as well as becoming firmer, and scarcely distinguishable in colour from the neighbouring parts; it appeared, therefore, a good time to put in the septum.

The promise of the success of this was not very great, as the centre of the lip from which the septum was to be taken was a white, bloodless-looking cicatrix after former lupoid ulceration. I only hoped that the deeper part would supply the necessary vascular supply. I cut out a triangular piece from the upper lip, left attached at its base just below the nose; this was turned up, the point cut off, and it was brought into apposition with the pared end of the septum, connected with the base of the flap of the new nose. When this was pared, it bled very freely, showing how completely organized with new blood-vessels the restored organ had become. Previously to cutting out the triangular portion from the upper lip, I divided the frenum from the gum, to enable me to turn up this new septum-bit well. The fresh ends of the two joined por-

Case of Richard Stark.—From a photograph taken before the Taliacotian operation.

Case of Richard Stark.—From a photograph taken about three months after the operation.

Case of Richard Stark.—From a photograph taken before the Taliacotian operation.



Case of Richard Stark.—From a photograph taken about three months after the operation.

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tions of the septum were kept in apposition by a single suture, the wound in the lip by a needle and twisted thread. The wound in the lip, from which the triangular bit was cut, united by the first intention, but not so the septum; as I had anticipated, the vital power was too weak, and it sloughed. In operating in cases of contraction after burns, where the parts are chiefly composed of a glazed, white, hard cicatrix, I have often seen sloughing of more or less extent to result. The entire new septum, however, was not destroyed; a small, thin band of union remained, dividing the aperture into two nostrils.

A few days after, I completed the operation by dividing the narrow portion of the flap where it was twisted in turning the raw surface towards the nose; the edges were pared, and the end was cut into a triangular shape, and then inserted into an incision made in the integument of the root of the nose; a transverse slip of sticking-plaster kept it down in its place, and it united at once.

The new nose was now finished, and looked very well; but there was a great tendency to contraction of the nostrils, which became very small, in spite of the insertion of small plugs of lint, which the boy managed very well himself: I therefore enlarged them so that they would admit freely about a No. 12 catheter; a piece of quill, wrapped round with lint, was kept in them till they should heal. After a few days I perceived that the thin septum did not stand the pressure between these two plugs, and that superficial ulceration existed, which went on, and shortly destroyed the new septum entirely. It turned out just as well, for the septum of the flap, taken from the forehead, was nearly long enough, and it was so turned back, and rested so close against the upper part of the lip, whence a natural septum takes root, that no one would observe any deficiency, and a sufficient transverse opening exists, through which he can breathe and discharge mucosities by blowing his nose.

The illustrations will best show the state of the patient before the operation, and also the appearance of the new member; the temperature, colour, and consistence are as satisfactory as the shape. As in the previous case published in this Journal, the profile is very hooked, but I dare say there will be found plenty of admirers of this form, the reasons of which I have already explained. I do not think that any one not acquainted with the circumstances of the case would consider the new feature as the manufacture of art: nature would be believed answerable for any faults of form. So far, it is satisfactory to

the poor patient, and frees him from the discomfort of impeded respiration, and the ridicule attaching to his former noseless condition. One fact is worthy of mention, as affording a fine example of the inexhaustible resources of nature to meet altered conditions of parts: as in an old, unreduced dislocation of the humerus or femur a new socket is formed, and the head of the displaced bone changed to meet its altered position; so here, though at first the flap from the forehead was so soft and thin that it moved to and fro in expiration and inspiration, yet after union had been confirmed, the blood-vessels supplied so hard a nutritious material to the new nose, that it became firm and thick, and not very much less consistent than the old. This may reassure those about to undertake the operation.

Art. XIV.—*On the Plague of Athens, as described by Thucydides.* By Jonathan Osborne, M.D., M.R.I.A., King's Professor of Materia Medica, Physician to Mercer's Hospital, &c. &c.

There are certain subjects connected with history which ever have been, and ever shall be, matters of doubt and controversy. They remain as open questions, on account of the impossibility of their ever being definitively settled. The authorship of Junius was one of these, and the nature of the disease commonly called the Plague of Athens, as described by Thucydides, is another. In the latter case the difficulty has arisen from the want of knowledge of medicine on the part of the historian, who was unable to select the appropriate and diagnostic symptoms of the disease, and who, in embodying the facts observed in a great number of cases in one general description, has presented a picture not resembling any one individual case. Hence much difficulty arises when we seek to identify it with any disease known to physicians. It has been generally agreed upon that it was the plague; but at the close of the last century Dr. Willan endeavoured to prove it to have been the small-pox, and Dr. Collier has the merit of originality in bringing forward, in a very interesting little work, a new opinion—that it was scarlatina.

In order to guide the judgment of the reader on this subject, he must be acquainted with the circumstances under which the disease occurred. The Athenians, at the suggestion of Pericles, had abandoned the surrounding country to the enemy; had sent away the sheep and cattle to Eubœa and other islands, and had crowded themselves into the city, so that, although a

few had house-room, yet the greater number must have been compelled to occupy the temples and vacant spaces; and at length many were forced to betake themselves to the long walls extending from the city to the port. The invasion of Attica by the Lacedæmonians and Bœotians, to the amount of 60,000 men, when the summer was at its height and the corn nearly ripe, caused a total destruction of the crops. It was in this state of things that the disease described by Thucydides in the following words, occurred[a]:—

"It was reported to have originated from Ethiopia, which is above Egypt; then to have descended to Egypt and Libya, and to have invaded a great part of the territory of the king (*of Persia*). Suddenly it fell on the city of Athens, and first attacked the inhabitants of the Piræus, so that the Peloponnesians were accused of poisoning the cisterns, there being no fountains there at that time. Thence it extended to the upper city, and many more died. I shall describe what took place, having been ill myself, and having seen others suffering from it.

"The year in which it broke out was, as admitted by all, peculiarly healthy with regard to other diseases, and all other illnesses tended to end in this; persons in perfect health, and without any known cause, were suddenly seized with violent flushings about the head (*της κεφαλῆς θέρμαι ἰσχυραὶ*), redness and inflammation of the eyes (*των ὀφθαλμῶν ἐρυθήματα καὶ φλόγωσις*), and in the interior the pharynx and the tongue became immediately bloody (*αἱματώδη*), and the breath unnatural and fetid. To this succeeded sneezing and hoarseness. Not long afterwards the affection descended to the chest with violent coughing, and whenever it settled at the heart it upset it (*ἀνέστρεφε αὐτήν*), and purgations of bile ensued of all kinds which have been named by physicians, and accompanied by great pain. A hollow hiccup (*λὐγξ κενὴ*) came on, in most cases giving rise to violent spasm, in some abating immediately afterwards, in others continuing for a considerable time. The body, when touched at the surface, was neither very hot nor pale, but reddish, livid, and covered with small vesicles and ulcers (*φλυκταίναις μικραῖς καὶ ἕλκεσιν εξηνθηκός*), but the interior was in such a burning state that even light garments or fine linen were not borne. They preferred to lie naked, and it was a great enjoyment to them to plunge into cold water. Many who were neglected ran to the cisterns, urged by insatiable thirst, and with the same result, whether they had much

[a] Thucydides, lib. ii. 48.

or little drink. Restlessness and want of sleep continued throughout. As long as the disease was in its highest state, there was no wasting of the body; on the contrary, it resisted this state of suffering beyond expectation. Thus, most perished on the ninth and seventh day by the internal heat, while they still retained some strength; or, if they escaped, then the disease, descending into the bowels, and violent ulceration taking place there, with a diarrhœa which could not be restrained, many subsequently died from weakness. The disease, which commenced in the head, passed through the whole body, and whoever survived its greatest dangers was liable to a seizure of the extremities, which left its marks behind, for it fell on the pudenda, the fingers, and toes, and many escaped after having been deprived of these parts. Some lost their eyes. Some on their recovery became forgetful of everything, and no longer recognised either themselves or their connexions.

"The character of the disease was beyond the power of words to describe; and it also came on each individual with a severity not in the usual course of human nature. It was manifestly different from ordinary maladies in this, that when a great number of the dead remained unburied, the birds or quadrupeds did not approach them, and if any tasted them, they died. As a further proof, an evident scarcity of birds of that kind ensued. The dogs also, on account of their living in society with men, afforded a still more evident illustration of the same fact.

"Such, then, was the general character of the disease, omitting the exceptional cases. None of the customary diseases occurred at the same time; whatever there were terminated in it. Death took place whether the patients were neglected or cared for. There was no remedy to be relied on, for what was beneficial to one injured another. No state of bodily strength, or of weakness, appeared to avail against it; and all succumbed, even though protected by every dietetical care. But the most dreadful part of the calamity was the state of mental depression which came on when individuals perceived themselves to be ill; for suddenly, losing all hope, they gave themselves up for lost, and thus died like sheep while taking it from one another in the course of their attendance on the sick, and thus was occasioned the greatest mortality; for when, through fear, they avoided each other, they died in solitude, and many houses were completely emptied in consequence of want of attendance; and if they lent assistance, they also died. In the same predicament were those who, influenced by feelings of duty and virtue, were ashamed

to spare themselves, and did not remit their attentions, even when the members of the family had been worn out by the groans of the dying, and overcome by the extremity of affliction.

"Still more, however, than these did they who had passed through it evince pity for the dying and diseased, as well from the knowledge they possessed of what they had to suffer, as from the fact that they themselves were in a state of safety, because the disease did not attack the same person a second time so as to prove fatal. Such were considered fortunate by others, and in the temporary excess of their joy indulged in the fanciful hope that they should never be carried off by any other disease.

"It was an additional grievance to the sufferers that a conflux of the inhabitants of the country into the city took place, which was equally prejudicial to the latter, for, in consequence of the want of house accommodation, many were compelled in the hot season to take up their abode in confined huts, and death spread without restraint. No decency or propriety was observed; the dead and dying lay heaped upon one another; while others, only half alive, were reeling about the streets and in the vicinity of every fountain, in order to indulge their longing for water. The sacred places in which they had fixed their dwellings were filled with the bodies of those who had died in them, for, under the pressure of the general calamity, men, not knowing whither to turn, disregarded every distinction between things sacred or profane. All the laws concerning burial of the dead were set aside. Each disposed of the dead of his household in whatever way he could effect it. Many resorted to shameful modes of getting rid of them, from want of those necessary materials which had been previously made use of. In some cases they even placed them on pyres which had been constructed by other parties, and, anticipating the real owners, applied the fire themselves: in other instances they took advantage of pyres already burning, casting their dead into the flames, and then took their departure.

"Chap. 58.—The expedition which was dispatched during the same summer, under Hagnon, against Potidæa and the Chalcidians of Thrace, was obliged to return to Athens without having effected any of its objects. For the disease spread not only among the men of this expedition, but it also attacked those who were already serving there, and who until then had been healthy; and the armament lost one thousand and fifty out of the four thousand chosen troops who had embarked a short time previously on this service."

It appears, according to the note by Dr. Collier (p. 74), that

the disease broke out in the second year of the war, and having raged with peculiar violence for a season, it became, during the third and fourth years, less frequent and less severe in its attacks; but in the fifth year it revived, so to speak, with something of its original intensity, and again declined, and eventually disappeared.

Dr. Willan's opinion, that the disease above described was the small-pox, rests principally on the eruption described as consisting of phlyctœnæ and small ulcers, the rest of the symptoms being either those common to fevers in general, or else such as are totally dissimilar to anything occurring in small-pox. But inasmuch as there is no description of small-pox, or of any disease resembling it, by the Greek or Latin physicians, up to the time of Rhazes, in the tenth or beginning of the eleventh century; and moreover, as it has not been noticed by any of the historians or satirical poets of Rome, to whom the disfigurement produced by it could not fail to supply subjects infinitely more inviting than many which they have adopted, it is, in the highest degree, improbable that they were acquainted with it, and also most improbable that a disease, so contagious, and so certain to be continued once it had commenced, could have existed at this early period, and then have disappeared during so many centuries afterwards.

The opinion advanced by Dr. Collier, that the plague of Athens was scarlatina, and which opinion he has advocated with much ability in his interesting work, lately published[a], reposes on the fact that scarlatina, in all its forms, and especially in that formerly known as angina maligna, or putrid sore throat, occurs epidemically; that death occurring on the seventh or ninth day resembles the mortality of scarlatina; that the disease never attacked the same individual a second time, and, above all, that it commenced in the mucous surfaces of the head and throat, and proceeded downwards in the same order as is so frequently observed in scarlatina.

Those coincidences, however, are not sufficient to prove an identity of the two diseases. Scarlatina, the plague, and small-pox, are all remarkable as usually attacking the same individual only once,—the first and last proverbially so; and out of 4400 cases of infection of the plague, Russel met with only 28 well-ascertained cases of recurrence; and although in all the three diseases exceptions to this generally received exemption

[a] "The History of the Plague of Athens," translated from Thucydides; with Remarks illustrative of its Pathology, by Charles Collier, M. D., F. R. S., Fellow of the Royal College of Physicians, and Honorary Fellow of the Royal College of Surgeons, London: 1857.

have occurred, yet not to such an extent in any one of them as to derange this point of resemblance between them. Again, they all come on epidemically; in all, death frequently occurs on the seventh or ninth day, so that this circumstance forms no distinctive mark of scarlatina; and the arguments of Dr. Collier, for fixing on scarlatina, being reduced to the presumed similarity between it and the description of the appearances in the mouth and throat, this description requires to be carefully examined.

The redness and inflammation of the eyes are not characteristic of scarlatina, but rather of measles, or of one of those forms of catarrhal fever which have so frequently been observed and described when occurring epidemically as influenza. The appearance of the pharynx and tongue, which Dr. Collier, along with other translators, has rendered blood-red (*αἱματώδη*), cannot be understood to consist merely in that colour, inasmuch as it is the natural colour of those parts in health. The word must, therefore, be taken in its original signification, 'bloody,' which I think will more clearly be proved to be the author's meaning when we proceed a little further in the consideration of the subject.

There is, however, no part of the description more unlike scarlatina than that of the appearances of the skin. Instead of the diffused redness and the peculiarly elevated temperature of the skin appertaining to that disease, we have in the description *small phlyctænæ and ulcers, and the external temperature of the body not hot compared with the internal heat.* Again, scarlatina offers no instances of loss of the eyes or of the extremities, or loss of the use of those parts, as ingeniously suggested by Dr. Collier. Neither has there ever been an instance recorded of epidemic scarlatina, or angina maligna, which did not chiefly attack children, of whom our historian makes no mention, nor of one attacking an army, as this disease did when, amongst the troops sent to Potidæa and Thrace, it cut off 1050 men out of 4000 within the space of forty days. Again, depression of spirits is no peculiar symptom of scarlatina in any of its stages, although occupying a prominent place in the description, which makes no mention of dropsical swellings, a well-known concomitant of that disease.

And now for the plague. On comparing this description with the numerous and close descriptions of the plague, as it occurs periodically in the Levant, drawn up by medical observers, we find much that cannot be reconciled. The distinguishing character of the plague is the breaking out of abscesses of the lymphatic glands within the first few days. With-

out this symptom there is nothing whereby we can diagnose between it and any epidemic typhoid fever. But in the description of our historian, the phlyctœnæ and the ulcers are mentioned as occurring in no one part of the body more than another, but appearing like an eruption or efflorescence (ἐξηνθηκός) on the surface.

When it is stated that the disease commenced in Egypt, and passed from thence to Libya and Persia, it certainly in this resembles the plague, which even at the present time proceeds from Egypt, where it is endemic, to commit its ravages in the Levant. But Egypt has been long recognised as the source of many epidemic fevers beside the plague[a]; and a great fact to be recollected is, that it has been proved that a temperate heat alone will tolerate the presence of the plague, and that whenever the thermometer ranges above 80°, or much below 60° Fahr., it is extinguished. It was observed at Aleppo during many years that it ceased when the heats of summer came on[b]; now the disease at Athens broke out in summer. Our author expressly informs us that *at the very commencement of the summer* the Lacedæmonians and their allies invaded Attica with two-thirds of their forces, as on the former occasion; and, after encamping, they laid waste the country. But they had not been many days in Attica before the disease began to appear among the Athenians, causing such a destruction of life as had never before been witnessed. This fact alone, that the disease broke out in summer, in the hot climate of Athens, is in direct opposition to experience of the usual time for the visitations of the plague, and *in limine* must suggest a doubt as to the disease described by Thucydides being that which is now called the plague.

We have, moreover, some valuable concurrent evidence concerning the diseases prevalent in Greece at this period. If the plague raged at Athens, it must be supposed to have also extended to the Grecian islands, between which and Egypt there was a constant communication. Now, Hippocrates was at this time, according to Soranus, in or about the thirtieth year of his age; he was practising in the island of Thasos, and in the adjacent regions of Thrace and Thessaly. Without placing any reliance on the fact stated by Soranus, that he once delivered Athens from a dreadful pestilence which was devastating the city, because it could not be the disease now before us, inasmuch as he states that it baffled all the efforts of the physicians, yet it can be proved that there

[a] Prosper Alpinus, i. 19. [b] Russel, "Natural History of Aleppo," p. 228.

was much direct communication between Thrace and Athens. Thucydides himself was possessed of property in Thrace. He ranked among the principal personages of that country[a], and appears from his own words to have at one time resided in Thasos[b]; therefore it cannot be denied that Hippocrates must have had opportunities of knowing all the circumstances relating to the disease at Athens. Now, in his "Epidemics[c]," which book contains the notes of his cases, and in the third book of which especially he has a distinct section, entitled 'the pestilential constitution' (*κατάστασις λοιμώδης*), there is first an account of the weather during the year, with a general epitome of the prevailing diseases. It concludes with his notes of fourteen cases occurring under his daily care in Thasos and the adjacent parts of Thrace. These cases, observed with a master's eye, and evidently reported with the utmost fidelity, prove that the plague did not at that time prevail in Thrace or the adjacent islands. They are as follows, viz.:—

No. 1, at Thasos, remittent fever; died on the 120th day.

No. 2, at Thasos, a remittent after child-bearing; died on the 80th day.

No. 3, at Thasos, a remittent; death on the 10th day.

No. 4, at Thasos, phrenitis; death on the 4th day.

No. 5, at Larissa, continued fever; death on the 4th day.

No. 6, at Abdera, acute fever, resolved with perspiration on the 4th day. Epistaxis.

No. 7, at Abdera, acute fever, resolved on 27th day with perspiration. Epistaxis.

No. 8, at Abdera, pleurisy, treated by venesection; resolved on the 34th day.

[a] *πυνθανόμενος τον Θουκυδίδην κτῆσίν τε ἔχειν των χρυσείων μετάλλων ἐργασίας εν τῇ περὶ ταῦτα Θράκη καὶ ἀπ' ἀυτοῦ δύνασθαι ἐν τοῖς πρώτοις των ἠπειρωτῶν.*—iv. 105.

[b] *Θουκυδίδην ὃς τάδε συνέγραψεν οντα περὶ Θασον.*—iv. 104.

[c] The edition which I possess of the first and third books of the "Epidemics" (Greek and Latin) was published in Dublin, in 1736, by Dr. Henry Cope. He was President of the King and Queen's College of Physicians, and State Physician; he was one of a family long seated at Loughgall, county of Armagh; lived in William-street; died in 1742, and was buried in St. Anne's churchyard. He was a pupil of Boerhaave. The work commences with an elegant Latin address to the Duke of Dorset, then Lord Lieutenant, followed by commendatory letters from Boerhaave and Bryan Robinson. Dr. Cope's object has been to illustrate the cases described in the "Epidemics," by comparing them with corresponding aphorisms and passages selected from the prognostics. The Greek text of those cases is well suited to illustrate how directly from nature they must have been taken. So far from being polished for publication are they, that, like the daily reports kept in a pocket-book, we frequently find adverbs without verbs, and adjectives without nouns. Cope's work clearly proves that the aphorisms and other didactic works of the Father of Medicine were not only founded on his actual experience, but were frequently exact epitomes of facts observed at the patient's bed side.

No. 9, at Abdera, continued fever, with deafness; repeated epistaxis; resolved on the 120th day.

No. 10, at Abdera, continued fever, after drinking and debauchery; resolved by perspiration on the 24th day.

No. 11, delirium tremens in a female, from excess of grief; resolved on the 4th day.

No. 12, at Larissa, acute fever in a virgin, with epistaxis, and first appearance of the menstrua; resolved on the 6th day.

No. 13, at Abdera, disease of the liver; death on the 34th day.

No. 14, at Cyzicum, puerperal mania; death on the 16th day.

Besides the above, the cases described in the first book of the "Epidemics," as well as in the first section of this third book, were almost all either remittent or continued fever, or dysenteries, and afford no indication of the prevalence of the plague, or of small-pox or scarlatina. They bear a very close resemblance to the diseases of the same climate, as described by modern observers. Thus Goodison[a] describes the variableness of the climate from the sirocco, the most prevalent disease being fever of every type, diversified according to the season: in winter and spring, intermittents; and in the early part of summer, continued fever, often accompanied by inflammations of the head, chest, stomach, or bowels; while in the latter part of summer and autumn they become remittent. In Minorca, almost similarly circumstanced in geographical position, the most frequent diseases are tertian fevers, sometimes beginning in a mild form, then running into continued forms, and terminating in dysentery[b].

Malaria is well known to occur frequently all along the western shores of the Morea. Lord Byron died of the remittent at Missilonghi. The whole plain of Thessaly is within the range of Greek malaria. The islands Lemnos, Naxos, and Paros, have been remarked as peculiarly pestiferous on account of these diseases. It is observed on board ship that the land-breeze often brings to the seamen the diseases of the shore, and that the character of the disease is often peculiar to certain limited localities, just as the Walcheren fever was totally different from that at the other side of the Scheldt[c].

In one passage[d], speaking of the pestilential year, Hippocrates mentions ulcers and swellings at the groins, but this is

[a] Ionian Islands: London, 1822.
[b] Cleghorn, Diseases of Minorca: London, 1751.
[c] Macculloch on Malaria: London, 1827.
[d] Hip. Epid., lib. iii. 16.

in connexion with ulcerations of the mouth, chronic ophthalmia, dysenteries, and various affections; so that during the time occupied by his work on the "Epidemics," the plague does not appear to have come under his notice. Did it prevail to any extent, however small, how could he avoid describing some case recognisable as such? This pestilential season was characterized by the prevalence of the sirocco, and thus he calls it ἔτος νότιον; but the absence of any case which can be identified with the plague, although the general mortality was great, sufficiently demonstrates that the disease was not prevalent at that time in Greece.

When we examine the description of Thucydides, we find no particular symptom to fix on as indicative of the plague more than of any malignant and fatal fever; but there are many which cannot be referred either to the plague or to any other epidemic fever on record. In the first place the bloody appearance of the mouth and tongue is not to be confounded with a merely deeper shade of colour of these parts, according to the views of Dr. Collier; for we have in Lucretius, whose description of the disease is mainly taken from Thucydides, the following passage, which gives an exact translation of the word αἱματωδη:—"Sudabant etiam fauces intrinsecus atro sanguine, et ulceribus vocis via septa coibat; atque animi interpres manabat lingua cruore"[a]. That this hemorrhage was accompanied by others would appear from the variety of the purgations mentioned, and not likely to come under the notice of a non-medical narrator, unless they were melenous, such being a necessary consequence of blood effused in the mouth and received into the stomach. That from the nose is specially mentioned by Lucretius[b]:—"Corruptus sanguis plenis ex naribus ibat;" as also that from the bowels:—"Et nigrâ profluvie alvi"[c].

The hiccup which occurred in most cases, so much so as to attract the attention of Thucydides, is characteristic of gangrene, and, taken in connexion with the bleeding of the parts within the mouth and throat, presents a striking picture of the low and congested state of the circulation belonging to scurvy. Hiccup is stated by Russel to be seldom observed in the plague at Aleppo[d], and is not frequent even in the lowest types of idiopathic fevers, unless when the abdominal viscera have been primarily engaged: that such was the case at Athens is evident. Again, the putrid fetor of the breath

[a] Lucretius de Rerum Natura, lib. vi. 1145.
[b] Lucretius, lib. vi. 1102.
[c] Lucretius, lib. vi. 1199.
[d] Russel on the Plague, p. 990.

from the commencement of the disease, along with the cool and livid state of the skin, the diarrhœa and the sense of internal heat, restlessness, and insatiable thirst, complete a picture of what we have sometimes occasion to witness in epidemic visitations of scurvy even in those countries, where the existence of hospitals and of improved diet and Christian philanthropy may be supposed always at hand to arrest it. At Athens all the population of the surrounding territory was pent up within the walls. The crops on which the subsistence of the inhabitants depended were destroyed; their diet must have been nearly restricted to what could be conveyed to them by sea. Their political state was at the lowest, with its necessary consequence of despondency and mutual mistrust and recrimination. It was almost inevitable, and no more than what was, *a priori*, to be expected, that scurvy should break out, and that, once having broken out, it should continue its ravages even after the city had been relieved. In many instances it may have assumed a chronic form, and this appears to have been the case with Pericles himself; for when he died of the disease he had no violent symptoms, but rather seemed to sink under a lingering affection, which gradually wasted his body and enfeebled his mind; and his enemies attributed the disease, which had been fatal to so many, to his mistaken policy in crowding the city with such multitudes of people, where they were confined in a state of total inaction during the heat of summer[a].

Neither the loss of parts, nor the loss of use of parts, has ever been enumerated among the consequences of the plague, or of any other febrile disease with which the description could correspond; but we find the same consequences described as resulting from scurvy when occurring in its severest forms, as on board ship, before the necessity of ventilation and of vegetable diet was known. This is the more remarkable, as they have led another non-medical author, but under very different circumstances, to describe them in very similar language.

In order to enable the reader to form his judgment on this point, I subjoin the description of the scurvy as it occurred on board Lord Anson's squadron during his voyage by the Straits of Magellan, in the year 1749. What renders this description peculiarly valuable for this comparison is, that the author was not a physician, but, like Thucydides, made his observations without any professional knowledge of disease, and that he has recorded those phenomena which produced the most striking effect on the mind of a non-medical observer. The author

[a] Plutarch, Pericles.

was the Rev. Richard Walters, chaplain to the squadron, and although possessed of a good English style, yet he nowhere appears to have indulged in classical recollections, and cannot be supposed to have had the description of Thucydides in view when he penned the following passage[a]:—

"Its symptoms are inconstant and innumerable, and its progress and effects extremely irregular; for scarcely any two persons have complaints exactly resembling each other, and where there hath been found some conformity in the symptoms, the order of their appearance has been totally different. However, though it frequently puts on the form of many other diseases, and is, therefore, not to be described by any exclusive and infallible criterions, yet there are some symptoms which are more common than the rest, and, occurring the oftenest, deserve a more particular enumeration. These common appearances are large discoloured spots, dispersed over the whole surface of the body,—swelled legs, putrid gums, and, above all, an extraordinary lassitude of the whole body, especially after any exercise, however inconsiderable: and this lassitude at last degenerates into a proneness to swoon, and even to die on the least exertion of strength, or even on the least motion.

"This disease is likewise usually attended with a strange dejection of the spirits, and with shiverings, tremblings, and a disposition to be seized with the most dreadful terrors on the slightest accident. Indeed, it was most remarkable, in all our reiterated experience of this malady, that whatever discouraged our people, or at any time damped their hopes, never failed to add new vigour to the distemper; for it usually killed those who were in the last stages of it, and confined those to their hammocks who were before capable of some kind of duty; so that it seemed as if alacrity of mind and sanguine thoughts were no contemptible preservatives from its fatal malignity."

"It often produced putrid fevers, pleurisies, the jaundice, and violent rheumatic pains, and sometimes it occasioned an obstinate costiveness which was generally attended with a difficulty of breathing, and this was esteemed the most deadly of all the scorbutic symptoms: at other times the whole body, but especially the legs, were subject to ulcers of the worst kind, attended with rotten bones, and such a luxuriancy of fungous flesh as yielded to no remedy. But a most extraordinary circumstance, and what would be scarcely credible upon any single evidence, is that the scars of wounds which had been for many years healed were forced open again by this violent dis-

[a] "Account of Anson's Voyage round the World." By Richard Walters, Chaplain. London: 1749. Page 101.

temper. Of this there was a remarkable instance in one of the invalids on board the Centurion, who had been wounded about fifty years before at the battle of the Boyne; for though he was cured soon after, and had continued well for a number of years past, yet, on his being attacked by the scurvy, his wounds in the progress of his disease broke out afresh, and appeared as if they had never been healed: nay, what is still more astonishing, the callus of a broken bone, which had been completely formed for a long time, was found to be hereby dissolved, and the fracture seemed as if it had never been consolidated. Indeed, the effects of this disease were, in almost every instance, wonderful; for many of our people, though confined to their hammocks, appeared to have no inconsiderable share of health, for they eat and drank heartily, were cheerful, and talked with much seeming vigour, and with a loud, strong tone of voice; and yet on their being the least moved, though it was only from one part of the ship to the other, and that too in their hammocks, they have immediately expired; and others, who have confided in their seeming strength, and have resolved to get out of their hammocks, have died before they could well reach the deck; nor was it an uncommon thing for those who were able to walk the deck and to do some kind of duty to drop down dead in an instant, on any endeavour to act with their utmost effort, many of our people having perished in this manner during the course of our voyage.

"After passing the Straits le Maire few were unaffected, and in April forty-three died of it on board the Centurion. In May we lost near double that number, and, as we did not get to land till the middle of June, the mortality went on increasing, and the disease extended so prodigiously, that after the loss of above two hundred men we could not at last muster more than six foremost men in a watch capable of duty."

In fine, taking into account the circumstances under which the disease described by Thucydides came on,—the crowded city, the necessary consequences of bad diet and ventilation, the depressed state of the public mind, and then the leading symptoms in his description,—the bleeding within the mouth, the phlyctænæ, which appear to have been petechiæ, the despondency, the tendency of all other diseases to assume the same low form of disease, and the transmission of it along with the naval expedition to Potidæa,—it appears to have been the same affection which has so frequently occurred when a number of human beings are confined under similar circumstances as at Breda in 1562, but which, from its more frequent occurrence at sea, has been termed sea scurvy.

Art. XV.—*Practical Observations on the Use of the Écraseur in Polypi of the Uterus.* By Robert Johns, A.B., M.B.T.C.D., L. and F.R.C.S.I.; Member of the Council of the Surgical Society of Ireland; late Consulting Accoucheur to St. Peter's Parochial Dispensary; Ex-Assistant Master to the Lying-in Hospital, Dublin; Vice-President of the Obstetrical Society of Ireland; Member of the Zoological Society of Ireland; late Chairman of the Midwifery Court of Examiners of the Royal College of Surgeons in Ireland; late Consulting Accoucheur to the Anglesey Lying-in Hospital; Examiner in Midwifery and the Diseases of Women and Children to the Royal College of Surgeons in Ireland; Consulting Accoucheur to the Peter-street Auxiliary Hospital, &c., &c.

When for the treatment of disease any new remedial agent has been devised, I conceive it to be a duty to test its value when opportunity is afforded, and to communicate the result of such investigation to the profession. Having recently witnessed three cases falling within the limits of this observation, and deeming that the details of two, with reference to the third, may not be uninteresting to the profession, I do not hesitate in making them public.

The malady of which I am about to speak is one which may consign its victim to the grave long ere its real nature has been ascertained, perhaps even suspected; but, fortunately for suffering humanity, once this has been disclosed, it is easy of removal, after which, in the great majority of instances, the return to convalescence is almost instantaneous; however, the issue in some few isolated cases has not been so fortunate, and fatal consequences have succeeded its abstraction, either from the fact of the reparative powers of the system having been reduced, by the exhausting nature of the complaint, to so low an ebb as to be beyond the power of reaction, from the complication of malignancy, or of intercurrent affections, or from the supervention of causes supposed by some to be the effects produced by the means employed for its cure.

Case I.—On the 3rd of September, 1857, Mrs. D., aged 43, a lady of a highly nervous temperament, consulted me, stating that she had been ten years married, but never pregnant; that up to the last two years she had enjoyed good health, her monthly periods having been perfectly regular and normal in every respect, but that ever since that time she had been subject to severe menorrhagia, often passing large clots.

The catamenia had returned every month, and lasted for a fortnight, being preceded for seven or eight hours by severe uterine pain, which for the last twelve months had been accompanied by sickness of the stomach, diarrhœa, and giddiness. During the intervals, the hemorrhage was replaced by a profuse greenish-yellow and slimy watery vaginal discharge, exhaling a very offensive odour. At this visit she had a general anemic and malignant aspect; was very much debilitated, and complained of giddiness, accompanied by fear, but not by headach; her appetite and memory were impaired; her temper had become irritable, particularly about trifles. She had lost flesh, did not sleep, occasionally lost her sight; suffered from palpitations and extreme nervousness; her bowels were habitually constipated; she had pain in her back and over the sacrum, uterine inertia and prurigo. She did not experience any weight or bearing down in the vagina, dragging pains in the groins, or any irritation in the neighbouring organs.

On making a vaginal digital examination, I found a fibrous polypus, about the size of a chestnut, projecting from the os uteri, which, by very slight traction, was brought down into the vagina; it was attached to the inner and anterior surface of the cervix, about an inch from the os, by a pedicle of about two inches long, and half an inch thick; the os was patulous, flaccid, and dilatable. Having determined to remove the tumour by "écrasement linéaire," I explained to my patient what I proposed doing, to which she willingly consented, at the same time saying that she would submit to any treatment to be again restored to health. As she expected to be unwell on the day or so following, I deferred any interference until after the period had passed over, but I ordered a tonic mixture, containing the ammonio-tartrate of iron, &c.

September 20th. The catamenia have ceased for three days. Ordered to have the bowels well freed, and to repeat the tonic iron mixture.

23rd. This morning I found my patient in a state of great excitement, and very hysterical. Not deeming her a fit subject for chloroform, I gave her some wine, which had the desired effect. Having then placed her in the position for lithotomy—which I consider preferable to any other in such cases, when operating on virgins, on females like Mrs. D., whose vagina is not much dilated nor very dilatable without inflicting much unnecessary pain, or on those whose os uteri is very high up—I then proceeded thus. Having gradually and gently intro-

duced the first two fingers[a] of my right hand into the vagina, and having found the polypus in the position before described, I passed a finger on each side of its pedicle, a little above its insertion into the tumour, and drew it down as near as possible to the perineum; having now replaced my fingers by the chain of the écraseur (which then surrounded the tumour), I shortened it until constriction was produced, and removed the polypus *very slowly* and steadily. There was not a drop of blood shed either during or subsequent to the écrasement, nor did my patient experience the least pain. A very trifling vaginal, discoloured discharge, caused by the debris of the pedicle, set in that evening, and continued for three days. Cold water vaginal injections, having been daily employed, on the fifth day she was up and about her house, and not a vestige of the pedicle could be discovered by the "toucher;" but as the os was still very patulous, and felt rough, an examination with the speculum was instituted, when an ulcer was seen extending about two-thirds around the os uteri, which yielded very quickly to a few applications of solid nitrate of silver, when the os closed up to its normal state. This lady called to visit me on the 3rd of December, when in appearance she was greatly changed for the better, as she had regained her natural healthy colour, and had lost the malignant aspect. She stated that her periods had become perfectly regular and painless; she had not vaginal discharge of any description, and, in fact, that she was in better health than she had enjoyed for years.

Case II.—On the 30th of December, 1857, I was consulted by Mrs. B., aged 48, a widow for nineteen years, of a bilious temperament. She mentioned that she had been married twenty-nine years since, and was mother of five living children, but had never aborted; that up to about twelve months ago her health had been very good, the catamenia having been perfectly regular and normal,—when their character became very much changed by coming on at intervals of a fortnight and three weeks, the discharge being profuse, accompanied by pain, and lasting for four days. That about six months past, at a period, severe hemorrhage set in, which lasted for three weeks; then ceased for six weeks, but returned, and was continuing at the time of her visit, severe bearing-down, without pain, having

[a] Cases sometimes arise in which the polypus is very large, and not manageable by the unaided finger: in such the vulsella are very useful, of which there are many descriptions, some single, others double; of the latter the blades are either permanently united, or so constructed for the convenience of application as to be attached or detached at pleasure.

occurred frequently on these occasions, expelling large clots, as it were the castings of a tumour; these attacks were preceded by sickness of stomach, giddiness, with great fear, flushings, and loss of sight. During the catamenial and hemorrhagic intervals a whitish-yellow and slimy discharge, having a heavy, bad odour, issued from the vagina. During this consultation she presented a highly anemic aspect; complained of palpitations, extreme nervousness, and debility; of weakness in the loins, bearing-down in the vagina, giddiness, without headach, loss of flesh, appetite, and sleep, irritability of temper, constipated bowels, and prurigo; at the same time mentioning that on that day a large quantity of water had passed with a gush *per vaginam*. On examining vaginally by the finger I found large clots, as before described, in the passage, the os uteri high up, very patulous, its labia being enlarged, rough, and congested, having a tumour about the size of a walnut growing from the internal and posterior part of the cervix, about half an inch from the os, by a pedicle an inch long, and a quarter of an inch thick, and hanging into the vagina. When about to leave my study, she fainted, but revived on getting a draught containing aromatic spirits of ammonia and sulphuric ether. Ordered a mixture consisting of sulphate of quina, sulphate of magnesia, dilute sulphuric acid, and compound infusion of roses: to be taken three times a day; arrangements having been made to remove the tumour the day but one following.

January 1st. My patient had not any sleep during the night from nervous excitement, hemorrhage still continuing with clots. Having given her some brandy and water, I placed her in the position for lithotomy, and operated as in the case of Mrs. D., taking some minutes from the tightening of the chain to cut through the pedicle of the polypus; not a drop of blood was lost during or after the operation, nor did she suffer pain. She then had an anodyne draught, having been much excited.

2nd. She expressed herself better; had not any return of the hemorrhage, but a trifling brownish-yellow discharge, caused by the remains of the pedicle, which was barely to be felt, was coming *per vaginam*. As she complained of headach and confined bowels, I ordered a pill containing blue pill, James' powder, and extract of henbane, to be taken at night, and a cathartic draught of infusion of senna, with rhubarb, and compound tincture of senna, in the morning.

3rd. Much better; headach removed; bowels freed.

4th. Vaginal discharge has ceased; no trace of the pedicle discoverable; os uteri closing; the speculum revealed ulceration on the entire of the posterior lip, extending into the os,

and the cervix was enlarged. Nitrate of silver, in the solid form, was freely applied; and a mixture, containing infusion of bark, syrup of bark, syrup of orange-peel, with citrate of iron and quina, was ordered three times a day. Under this treatment she got perfectly and rapidly well, and is now in better health than she remembers to have enjoyed.

About a year since I assisted the late Dr. Monahan to remove a polypus by the écraseur from the cervix uteri of Bridget Marks, an inmate of the North City Poorhouse, whose case is published in the Dublin Hospital Gazette for January 15, 1857, and although the tumour weighed three ounces, and was fully eight times larger than either of those taken from my patients (the subjects of this communication), still each of them lost by far a greater amount of blood than she did. But the size of the tumour seems not to exercise any effect in regulating the amount of hemorrhage, except, perhaps, in the inverse ratio; for we have on record cases in which almost fatal bleedings have occurred, although the polypus was not larger than a currant; indeed, every practical accoucheur must be impressed with the truth of this assertion from his own experience. Perhaps ulceration of the os or cervix uteri coexisted where this symptom was so severe, as it did in my cases; whereas it was absent in the case of Marks. I lay stress on this complication, as I believe it to be (as I have before stated elsewhere), one of the sources of hemorrhage, and a fruitful one too, in polypial disease.

Marks never, at any time, suffered from weight or bearing down in the vagina, or on the rectum, although the tumour was so large, and so much protruded externally, as to have been mistaken for procidentia uteri when I was brought to see her. Yet with Mrs. B. this symptom was most distressing.

In all these cases the tumour was separated close to its pedicler attachment, because, if happily hemorrhage did occur, the remnant of the stalk could be easily seized and ligatured; besides, by thus acting the cure is not interfered with, as the portion left behind (as is well known) either comes away in the discharges, or drops off in time from its insertion to the uterus, as the cord does from the umbilicus of the infant.

I think that I have fully demonstrated the utility of the écraseur in the disease now under consideration, which plan of treatment, I should say, according to my experience, is preferable to all others hitherto employed, as being more general in its adaptation; however, I am not inclined to go so far as to discard all other means of cure, such as torsion, deligation, and excision, for I conceive that we occasionally meet with

cases in which each of those may be advisable; for instance, who for an instant would think of ligaturing, excising, or écrasing a small mucous polypus at the os uteri, when he could so easily twist it off with a long forceps[a]? Again, there are very large polypi, having very thick pulsating pedicles, evidently from the presence of large arteries, which perhaps would be more safely removed by deligation, or by excision with previous deligation. Some object to excision from the dread of subsequent hemorrhage, but as I never met such a consequence, I believe it to be a very rare one; at all events plugging will quickly control it when it occurs; still, let those who apprehend such an occurrence throw a ligature around the growth for twelve hours, or even less, before using the curved knife[b], or the curved scissors[c], and their fears will be groundless. This combination of appliances has been employed by me in this disease, as well as in amputating the cervix uteri, and has been recommended by me in my published monographs on that subject; others dread the supervention of phlebitis, or such effects, from purulent absorption, after the use of the ligature, which in my opinion is highly improbable, unless in subjects previously disposed to them. Notwithstanding, if we have a patient suffering from polypus of the womb, who has been so much debilitated by the discharges that the loss of a very small extra quantity of blood might be fraught with danger, and whose condition from hemorrhage, &c., might predispose to purulent absorption, I think with her the knife and the ligature ought to yield to the écraseur, provided it be judiciously employed; that is, very *slowly* and steadily used, for we must not be too anxious to finish the operation in a hurry, merely to show our dexterity, as want of attention to this fact will produce the supposed evils of the other operations: by being precipitate we separate the tumour before condensation of the tissues takes place, or the vessels are closed, which may cause hemorrhage at the time of operating, and purulent infection at a later period. Not very long since a lady in Liverpool lost her life after the use of this

[a] The forceps which I use was made for me, according to my own directions, by Mr. Millikin, formerly of Dublin, now a partner with Mr. Begg, of London. It is eleven inches long, and very slightly curved towards its pointed extremity, and is furnished with a spring racket in the handle, to prevent the body grasped by it from slipping away; in other respects it is like the ordinary polypus forceps.

[b] My knife is eight inches long, four of which enter into the blade, which is curved laterally, cutting on its concave edge.

[c] The scissors employed by me is nine inches long, and curved on the flat for four and a half inches.

instrument; and I am credibly informed that in one of our hospitals in this city much hemorrhage followed on its use.

Since writing these few remarks my attention has been drawn to a very valuable paper in the "Dublin Quarterly Journal of Medical Science" for August, 1857, on the use of the écraseur, from the pen of Mr. O'Doherty, surgeon to St. Vincent's Hospital, in which he states that M. Chassaignac's rule to guard against hemorrhage was "to allow, in the case of ordinary vascular tumours, an interval of fifteen seconds to elapse between each movement of tightening; in other words, between each 'clicking' sound of the instrument, which indicates so much 'paying in' of the chain; in some cases he has an assistant to note the time for him." Thus it is obvious that the learned inventor of the écraseur[a] is by far more cautious in its use than some of his disciples.

The following facts, I think, are fairly deducible:—

1. That vaginal hemorrhage, continuing for any length of time, being accompanied by clots of blood, assuming particular shapes, and having been preceded by an increase of flow at the catamenial periods, is strong presumptive evidence of the existence of polypus of the uterus.

2. That the situation of the tumour, whether in the uterus or descended into the vagina, does not seem to exercise any effect in increasing or decreasing the hemorrhage.

3. That the amount of blood lost in this disease is not in proportion to the magnitude of the tumour.

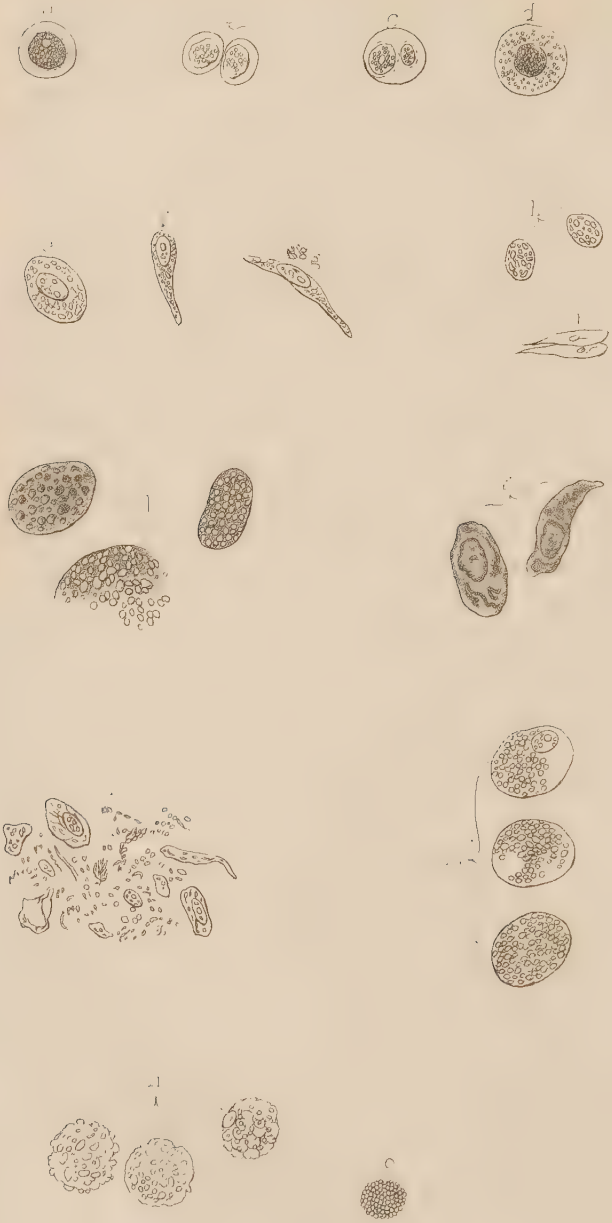
4. That ulceration of the os or cervix uteri is a very fruitful source of hemorrhage in polypial disease.

5. That it is not by any means necessary to draw the tumour externally to the vagina, in order to remove it by linear écrasement.

6. That there is no advantage derivable from écrasing the pedicle high up.

7. That the écraseur is a valuable instrument if properly employed; that is, very SLOWLY and *steadily;* but, if not, its use is very doubtful, if not hazardous.

[a] There are many modifications of Chassaignac's écraseur: amongst others is that of M. Charrière, which is so constructed that it can be made to act as a curved or straight instrument, at the will of the operator, by means of movable ends; the advantage derivable from this addition is said to be experienced when écrasing in canals like the vagina; however, I am of opinion that any dexterous man could operate as well with the straight one. I prefer M. Chassaignac's instrument, as, from its construction (both ends of the chain being permanently attached, thereby causing a regular alternate movement for its action), it insures a more slow, steady, and safe operation.



Art. XVI.—*On Cancer*[a]. By Maurice Henry Collis, M.B., M.R.I.A., F.R.C.S., Surgeon to the Meath Hospital and County Dublin Infirmary; and Examiner in Surgery to the Queen's University in Ireland.

There is no point connected with this fearful disease, nor is there any mode of investigating it, that the practical surgeon can afford to neglect. Great advances have been made, in the course of the last ten or fifteen years, in the study of cancer; much new light has been thrown upon its structure, some upon the mode of its development; and, as correct diagnosis always aids practice, I may add, some upon its treatment. The subject, however, is by no means exhausted, nor is there an unanimous acceptance of the results by the profession. There are many questions relative to cancer still unsettled; and upon one or two of these it is my purpose to make a few observations in the following pages:—

First, as to its nature.

Secondly, as to its local or constitutional origin.

Thirdly, how far it is amenable to treatment.

Is it possible, in the first place, to define cancer? I think it is; and that its essence is to be found, not in a mysterious malignancy of character, which, like a phantom, evades our grasp when we attempt to seize it, but in its anatomical structure; not indeed in the coarser features which characterize cancerous tumours, and which they possess more or less in common with other structures the result of disease, but in a peculiar element which is found in cancerous growths, and in them alone. In one word, I adhere to the cancer-cell (in its various forms) as the essential difference or special element, by the presence or absence of which we are alone able to pronounce with certainty on the cancerous or non-cancerous nature of any tumour.

Before endeavouring to refute the arguments that have

[a] I have endeavoured to group in the accompanying lithograph the cells most commonly met with in scirrhus and encephaloid cancer. Although several are copied from the works of Paget, &c., they represent in every instance cells such as I have myself seen, and have been copied because they are more accurate or typical than my own drawings. They are all magnified 450 diameters. Lower powers than this will fail to give correct ideas of the structure of cancer. (a) Single encephaloid cell. (b) Two smaller encephaloid cells. (c) Cell with two nuclei. (d) Large-sized cell from scirrhus. (e) Round; (f) caudate; (g) fusiform—cancer cells from scirrhus.—(Paget.) (h) Free nuclei.—(Paget.) (i) Free nuclei, shaped like melon-seeds, from encephaloid of lower extremity. (j) Melanotic and oily cancer cell and fragments. (k) Two cells.—(Velpeau.) (l) Withered cells; (m) oily cells.—(Paget.) (n) Oily cells.—(Walshe.) (o) Compound granular corpuscle of Gluge (inflammatory, not cancerous).

been brought forward against this view of cancer, I shall briefly lay down the peculiarities of this cell, as they are to be found in the works of authors who have written on the subject, and as verified and, it may be, extended by my own repeated observations. During ten or twelve years I have made it a point to examine every cancerous or cancroid growth with the clinical history of which I was acquainted; and as far as courtesy to others would allow, I have purposely avoided giving an opinion on the microscopic nature of tumours which I had not followed at the bedside. I therefore have a confidence in my own conclusions, because they have been arrived at by a process of combined clinical and anatomical investigation, and because I had at first no leaning towards one side or other of the microscopic controversy.

When I say that I had no prejudice in favour of the cell theory, it is but right to add that certain arguments, *à priori*, would strongly lead one to consider the existence of a special element a matter of great probability. If a fatty tumour is found to contain fat granules; an enchondroma, cartilage cells; a fibrous tumour, certain forms of fibre;—it is not unreasonable to expect that tumours of so distinctive a character as cancers would present a specific organization. Some confusion of ideas, however, at once arises from this point of view, for are not schirrus and encephaloid both cancers? and yet in external characters they are often as unlike as any other two morbid growths we could name. When considering the question of the local or general nature of cancer by-and-by, I hope to be able to explain that dissimilarity of external character is consistent with fundamental identity of structure.

Cancer is a morbid infiltrating growth, the tendency of which seems to be sooner or later to destroy all tissues with which it comes in contact. This morbid substance is only discoverable, at present, in the form of a peculiar nucleated cell; neither chemistry nor the microscope have traced the *materies morbi* higher than this cell: it may or may not exist as a peculiar element in the fluid that forms the plasma of cancerous tumours, or it may even exist in the blood; but of this we have no knowledge, however we may speculate or reason from analogy one way or the other. With the same reservation I would say that cancer has no specific stroma. The stroma of a cancerous tumour appears to be generally made up of the fibrous tissues of the organ in which the cancer cells are deposited. It often, if not always, presents features of considerable clinical value; it often shows the rate of the development of the tumour, and forms a gauge of its destructive power. It may be distorted or

atrophied, or it may be hypertrophied, either by the organization of plastic effusions, or by a process of protective thickening excited by the presence of the cancer cells; but it is not cancer; and in a scientific, if not in a clinical point of view, we must be careful to distinguish between the essence of a disease and its effects.

The characters of the cancer cell are as follows:—

In *size* it varies from the 1-700th of an inch (d) to the 1-1600th (b); the variation depends on the age of the cell and the rapidity of the tumour's growth. The cells of encephaloid are rapidly formed in great numbers, and are smaller than the more slowly developed cells of scirrhus; in this particular cancer cells resemble epithelial cells, which are round and small where moisture abounds and favours rapid growth, and which are large and flattened out according as circumstances deprive them of their moisture.

The cell wall is delicate, often not to be perceived but by an oblique arrangement of the light, or by touching with very weak vinegar and water. Acetic acid entirely dissolves it. The typical form is round or oval; it is frequently flattened out or pressed on by neighbouring cells, and rendered polygonal; whether by pressure, or more likely by tendency of growth, it is often caudate (f), and occasionally the tail-like process is elongated into a kind of fibre (g). In the same specimen great varieties of shape and even of size will be observed; this multiformity is a special characteristic of the cancer-cell.

The nucleus, which is contained in the cell, is much less variable in size and shape: it is large compared with the nuclei of other cells; it is more or less oval (h, i); the long diameter is about 1-2500th of an inch; its margins are well defined, much darker than the outline of the cell wall, and rendered more distinct by the addition of dilute acetic acid; the acid not only dissolves the cell wall and granular blastema, but also, if not too strong, it acts directly on the nucleus, perhaps, by coagulating the albumen which it contains. The addition of diluted acetic acid, therefore, renders the nucleus, both relatively and positively, more distinct. Occasionally a second nucleus is perceived in a cell (c); care, however, must be taken not to mistake the accidental superposition of one cell over another. More than two nuclei are not common, and where more than one exist, they are generally somewhat dwarfed. Nuclei are frequently found free, especially in scirrhus; they resist destructive agents, including decomposition, better than the cell wall. Hence they are often found surrounded by the broken fragments of the cell wall (l); and as this form of degeneration of

the cell occurs more constantly in scirrhus, this is probably a sufficient reason for their being more numerous in it.

One or more *nucleoli* are generally found in each nucleus, similar, so far as present observation has gone, to the organic granules, which abound in all organizing blastema, except that they are sometimes rather larger and more sharply defined.

Besides these, the cell contains fluid almost transparent in recent cases of encephaloid, but more or less granulated under other circumstances, and similar in all respects to the blastemal fluid in which the cells float.

Such is the typical cancer cell, the special element of cancer. In every specimen, however, that we examine, variety of form and outline will present itself; cells will vary in size, nuclei will be found dwarfed, and broken fragments, as well as natural structures, will be mixed up in the field. A sufficient number of typical cells will, however, be found to decide the question, and a little practice enables us to overcome these difficulties. The degeneration of the cancer cell is a common cause of difficulty in detecting its presence. Its destruction appears to take place in two distinct ways: it either dries up and falls to pieces (l), as all cells do when dried, or it becomes the seat of an oily degeneration (m). In the former case the nuclei are seen more or less perfect, surrounded by the dust and fragments of the cell wall and its granular contents; in the latter, the cell wall is rendered more distinct, while the nucleus is proportionably obscured by the adhesion of minute drops of oil to the inner surface of the cell wall. Cells, in this state of fatty degeneration, must not be confounded with the compound granular cells (o), which are sometimes found mixed with cancer, as they are wherever active inflammation is set up from any cause. The compound granular cell is made up of an aggregation of organic granules, and has a mulberry outline; it is round, with granules of an even size, projecting on all sides, while the oily cancer cell is smooth, and contains the oil-drops in its inside; these drops, also, are of all sizes, and the cancer cell (n) is larger than the compound granular cell (o). Cells only half full of oil (m) will be found in cancer, which will assist the diagnosis; but it is often difficult to distinguish them, and the presence of oily cancer cells in tumours has given rise to mistakes of diagnosis which have done much to retard the general acceptance of the cell theory, as we shall see presently.

The reasons why the cell theory has not been as yet universally adopted are various. Foremost stands the confusion of terms, which in all disputes is the fruitful parent of contention and error. Some surgeons conceive that it is enough

for a tumour to be destructive of the neighbouring tissues or of life to give it the name of malignant, and they are right; but when they make the words *malignant* and *cancerous* convertible terms, they are not right. Cancers may remain for years slowly growing, and the patient may die of other disease without at any time presenting a symptom that can be called malignant; and, on the other hand, many diseases are clinically malignant that cannot be called cancers. Velpeau, for example, triumphantly brings forward a case of sarcocele of the testis, the removal of which was followed by the growth of large cerebriform masses in the abdomen. Microscopic examination found no cancer cells either in the primary or secondary growths. Yet, because the disease returned in this malignant manner, he begs the question, and pronounces the disease to be cancer. Within the last few months a somewhat parallel case has been brought under my notice, which would show the fallacy of his argument. A gentleman who had been treated with apparent success for scrofulous disease of the testis died in a few months of jaundice, and his liver was found infiltrated with innumerable masses of imperfectly organized fibrinous material, so like, in its coarser characters, to cancerous infiltration, that a person not accustomed to view things as closely as a microscopist habitually does, might well be excused for supposing it cancer at the first glance. Yet the clinical history and the examination of the body left no doubt that the disease of the testis was of a strumous nature; and a sufficiently careful inspection of the infiltrations in the liver disclosed points of difference from cancer, even without the microscope. There are, without doubt, cases in which other diseases, such as fibro-plastic tumours, or scrofula, especially when mixed with syphilis, run a most malignant course, and bear a close resemblance to cancer; but we should not be led by these casual resemblances, however close, into the unscientific error of confounding things fundamentally different. It would, therefore, be wise to restrict the terms *malignant* and *benign* to the clinical features of cases, and not to use them as implying any peculiarities of structure.

Other causes for the rejection of the cell theory of cancer are to be sought in the prejudices of those who have acquired their knowledge of tumours without the aid of the microscope. A similar prejudice, for a time, excluded the stethoscope from general use, and prevented many from ever employing it. And as, in the case of the stethoscope, men were to be found who wielded it with solemnity, and yet knew not how to use it, so it has turned out with the microscope. In the great discussion upon the subject at Paris, the year before last, men took part,

who, by their own confession, were from ignorance incapable of deciding the points at issue. These men most unjustly threw the weight of their names at random into one scale or other, and damaged the cause of science by unfair opposition, or ignorant support. Points such as these must be ultimately decided by those who bring to the discussion extensive observation, an honest purpose, and an unprejudiced mind; and, except in rare instances, we seldom find men, however honest, free from the prejudices of early training, or willing, even if they have time, to commence a course of observation that requires as much labour to master it as the acquisition of a new language would.

Another reason for the slow acceptance of the cell theory is to be found in the errors of the first discoverers and promulgators of the theory. In the first flush of discovery, before anything like an adequate number of observations had been made, they attempted to lay down absolute laws from insufficient data. Hence they fell into errors that were eagerly seized on by their opponents, and used, not unskilfully, to overturn, if possible, the entire system they had built up. I may be allowed to quote the remarks of Mr. Gamgee upon this point; they are most apposite; and, coming from one so favourably known for enlarged views on the science and practice of surgery, they must command attention.

Mr. Gamgee is examining into one of those cases which had been erroneously classified, and in which the error had given rise to much discussion. The complication of cystic disease had rendered the diagnosis of cancer difficult before operation, and the apparent absence of the cancer cell had caused too favourable an opinion to be pronounced after the tumour had been removed and submitted to the microscope. He says[a]:—"Before operation there was some doubt as to the nature of the tumour; the softish points detected on palpating its surface were regarded as cysts, and, notwithstanding the patient's age, the severe gnawing nocturnal pain, retraction of the nipple, and enlargement of the lymphatic glands, the cystic enlargements were regarded as indications of the benignancy of the tumour, and it was diagnosed for cystic sarcoma. No cancer cells appearing upon its being submitted to careful microscopic examination, the diagnosis of benignancy was held to be indisputably confirmed, and accordingly, a favourable prognosis was pronounced. No notes were taken of the microscopic examination, but I perfectly remember observing at the time, that

[a] "Researches in Pathological Anatomy and General Surgery." By Joseph Sampson Gamgee. Page 62.

all the specimens which I placed in the field contained a large number of compound granulated corpuscles. . . . Repeated and progressively more enlightened microscopic inquiry has inspired me with the belief that this tumour probably contained cancer cells, but that, from their being stuffed with fat molecules, we mistook them for compound granular corpuscles." In the early part of my microscopic examinations I held the opinion that the presence of the compound granular cell was sufficient to prove the tumour to be cancer. We were both, I believe, in error; the real explanation being that at which we have both independently arrived, namely, that the compound granular cell, which is a constituent of highly inflammatory effusion, and the cancer cell in a state of oily or fatty degeneration, resemble each other so closely, that it is often impossible to assign individual cells to the one or other class. In any given specimen, however, some cells will be found sufficiently well marked to enable us, now that attention has been been drawn to the fact, to say what they are. In my "Clinical Lectures on Cancer," published in 1855 in the "Dublin Hospital Gazette," I described a means of distinguishing the two by their outline and size, and I have repeated the description in the course of this present essay. Mr. Gamgee gives us another test, namely, the action of ether, which dissolves the oily cancer cell; and it will, I apprehend, be found, that it has no action on the compound granular cell.

The fact that such an error has occurred in our early investigations, now that it has been discovered, as other errors probably will, upon extended investigation, enables us to comprehend at once the few exceptional cases which have stood in the way of the reception of the specific cell nature of cancer. If any of my readers are still unconvinced, I must refer them to Mr. Gamgee's essay on the subject, in which all these cases are categorically examined into, and their proper place assigned them.

I have lately had a case under my care which illustrates the difficulty of making the diagnosis of cancer when complicated with cysts, and the value of these cells in settling a doubtful question of diagnosis. The particulars are as follows:—

In February, 1857, a lady consulted me about a small swelling in the upper part of her left breast: it was movable under the skin, and attached to the gland, but standing up from it; it was hard, and irregularly nodulated; it at once gave me the impression of being a scirrhous cancer. There were severe sudden, sharp darts of pain through it; blue veins ramified over the surface; the nipple was not retracted, either then or sub-

sequently, but a discharge of bloody serum came from the nipple almost from the first; this discharge was greater after handling the breast. The size of the tumour, when I first saw it, was about that of a marble; it had been remarked six weeks before I was consulted about it. The tumour increased very slowly; but a gradual increase in the elevation of the surrounding parts was perceptibly marked in the month of June, and by the beginning of September the original tumour was almost lost in the increased growth of the gland; the surface of the breast had, by degrees, acquired a granulated feel, as if shot-grains were imbedded in the gland. The pain was still referred to the site of the original tumour; the integument was unaltered in texture, and was not unnaturally adherent to any part. The lady's age was 38; she had one child only, aged about ten years; she was very fat, and suffered much at one time from passive congestion of the lungs and palpitations; but for some time previous to the appearance of the growth she had enjoyed good health, with the exception of some trivial uterine disturbance, for which she had the benefit of Dr. Johns' advice; and we were both of opinion that it had nothing to do with the tumour of the breast.

Various treatment was adopted, including the iodide of arsenic, and iodine and iodide of potassium, externally and internally, without any effect. Fomentations which were used at an early period produced an increase of pain and discharge from the nipple. The lady's general health was good, but the presence of the diseased growth gave her much uneasiness; and, after consultation with Mr. Cusack, I removed it on the 17th October, at the "Pension Médicale" in Charlemont-street. All progressed favourably, and the wound had cicatrized early in January of this year.

On examination, after removal, the whole gland was found full of cysts, varying from the size of a pin's head to that of a small haricot bean; these cysts were mostly full of an oleo-albuminous fluid; some few had three or four secondary sacs inside them attached to each other and to the wall of the cyst by a hollow pedicle, by means of which each of these secondary cysts communicated with all the others enclosed in the same primary cyst; the size of these miniature bunches of grapes was about that of currant-seeds; the contents of these little secondary cysts was of a blood-red colour, and consisted of blood and simple granular corpuscles. Some of the cysts communicated with the milk ducts, and the flow of sanious fluid from the nipple was thus accounted for. The bulk of the tumour consisted of these cysts, the milk ducts, and fat. At the seat of

the original tumour, at the upper part of the gland, the cysts were small, and closely set in a dense, tough mass, resembling lardaceous scirrhus.

No cancer cells of the ordinary type could be found in the fluid which flowed from the cut surface of this part, but large granular bodies (j), deeply stained with a dark brown pigment, floated about in abundance. The memorandum I made on the spot states:—I conceive these to be cancer cells filled with oil-globules, and so impregnated with black matter as to have the nucleus obscured: the impossibility of measuring the nucleus is, in fact, the only element of doubt in this case; and this doubt is done away with in my mind by the history of the case. I recollect at the time testing these bodies with various reagents, and being puzzled by the fact of ether decomposing them into separate oil-globules (j). Mr. Gamgee's paper has since that cleared up this difficulty. Some of the cells were the largest I ever met with, being not less than the 1-400th of an inch in diameter; this great size, the action of ether upon them, and the irregular size of the oil globules, sufficiently distinguished them from the compound granular cell of Glüge.

The microscopic examination of this case enabled me to corroborate the view I had formed of the original tumour, and cleared up all doubts as to the nature of the subsequent enlargement of the whole gland. I have no hesitation in pointing to it as a case of the combination of cystic with cancerous degeneration, the cysts spreading more rapidly through the gland than the cancer, in accordance with their usual respective rates of growth. The combination renders the prognosis most unfavourable.

It may not be amiss to state here the clinical features which, viewed retrospectively, seem to warrant this conclusion, even apart from the light thrown on them by the microscope; premising, however, that what would be but conjecture without the microscope is turned into certainty by its aid.

1. The occurrence of severe darting pains at an early date, and their limitation all through to one point in the breast.

2. The presence of a distinct though small elevated tumour in this point; the tumour being of stony hardness and nodulated surface.

3. The subsequent elevation of the entire breast almost to a level with this tumour; the surface of the gland becoming at the same time granulated.

4. The absence of retraction of the nipple, which was at some distance from the first tumour.

5. The sanious discharge from the nipple, produced, as I believe, by the communication of some of the cysts with the milk ducts.

Among the errors of the earlier investigators is the including of epithelial growths among the group of cancers. Similar as they are in many of their features, and in many of their disastrous results, they still present sufficient points of clinical difference to entitle us to rank them apart, now that their anatomical structure is revealed to be so essentially different. The refutation of this error I must also postpone until discussing the origin and growth of cancer.

For the present I shall conclude with another quotation from Mr. Gamgee's essay:—

"There can be no question that in this discussion zealous enthusiasts, no less than retrograde prejudice, have tended to vitiate the conclusion. Impartial consideration of all recorded facts must convince any one that, as all the circumstances of a case require to be taken into account for the formation of a correct diagnosis, the structure of disease must hold a foremost rank in the inquiry. In determining it, the microscope is of great service; hence its title to be regarded as a first-class auxiliary. It is very probable errors will be committed with it, even after much more accurate study; but this will in no way affect the truth of the proposition, that the liability to error is greatly diminished by the light with which it guides the clinical student. Truly, as the microscope, in the majority of cases, only comes into play after the intervention of the knife, it is not a material aid to surgical treatment; it is so, nevertheless, in the relief of moral suffering; for I can conceive no greater felicity to all concerned than the being able to prognosticate, with almost perfect certainty, the benignancy of an excised growth, about the malignant nature of which doubts would very often be entertained but for the microscope's aid. But let any one, still disposed to question the microscope's worth to the practical surgeon, cast a glance back half a century to John Abernethy's attempt to classify tumours; let him endeavour to discover the causes of the great progress which has been made in this important department of pathology; if impartial, he will find it impossible to refuse a lion's share in the good work to microscopic inquiries." To this I would only add, let any one accustomed to examine tumours under the microscope say how much he owes to it for having his attention drawn to minute particulars of structure and consistence, which in turn have thrown light on the physical signs;

how often has it enabled him to classify what would otherwise have been a confused mass of information, and to utilize it on subsequent occasions. None, in short, who have not used the instrument long and often can appreciate the practical advantages it confers.

(*To be continued.*)

Art. XVII.—*Strychnia and its Uses.* By H. R. de Ricci, Esq., Ballymahon.

(*Continued from* p. 50.)

This was an instance of hysteria in the male. I call it hysteria because I do not intend to manufacture a new name, and I do not know of a better to express the condition of the patient whose case I am going to relate. Besides, several cases of *hysteria in the male*, as it is called, have been recorded; and I, therefore, have a precedent for its adoption; but no matter how great a misnomer, it will serve to show that the group of symptoms generally known by the name of hysteria may occur quite independent of any affection of the womb, and are, in fact, only the result of perverted, diseased, or deficient nervous action.

A gentleman, of about forty years of age, married, and father of a large family, of studious, but at the same time not over sedentary habits, a great smoker, and rather free drinker, was several years ago attacked with what was considered by many physicians to be incipient softening of the medulla. The first thing noticed in him was a sort of dragging in the left leg when walking, and an apparent lowering of the left shoulder; in a few months a sort of forgetfulness of passing events was superadded to the previous symptoms. He became extremely irritable, and the slightest noise would put him into the greatest paroxysms of nervous excitement, during which tears and laughter would alternate with each other. He now began also to complain of pain in the spine, about the region of the fifth and sixth cervical vertebræ. The treatment which had been adopted was,—free cupping over the painful spot, active purgation, and caustic issues in both arms, with total interdiction of tobacco and all stimulating drink. Under this treatment, instead of improving, he grew worse; he became so feeble that he could only walk a few steps, and had to be lifted on his pony every time he wished to take a ride. His bowels never acted except by the aid of the strongest purgatives, and not

unfrequently the catheter had to be resorted to for the purpose of relieving his bladder. He often burst into paroxysms of uncontrollable laughter, without any cause, which invariably terminated in a copious flow of tears. He never slept more than two or three hours out of the twenty-four, and his appetite was completely gone.

Such was his state when he first placed himself under my care about eight years ago. I had ample opportunity of seeing him often, and studying his case; and, after mature deliberation, I came to the conclusion that if there was such a thing as *hysteria in the male*, this was surely an instance of it. Acting upon this conviction, and being anxious rather to strengthen the patient than weaken him, I at once closed up the issues, allowed him free nutritious diet, with a moderate supply of bitter ale, and his favourite pipe after dinner. These three changes acted most satisfactorily. First of all they established me firmly in his estimation, a point of the very greatest moment in the treatment of disease, as no patient is likely to improve satisfactorily under the care of a physician in whom he places no trust; secondly, these allowances served to fill up a vacuum, a want, which had been caused by the *too sudden* interdiction of long accustomed stimuli. I also ordered him to be sponged every morning with tepid water, and then smartly rubbed, in order to induce active capillary circulation. (I had omitted saying that the heart's action was sluggish and irregular.) Purgatives were forbidden, and enemata substituted *when required*, for the tobacco pipe had very much removed the torpidity of the bowels. The only medicine he took was infusion of quassia three times a day, and a liniment containing aconite and belladonna was rubbed every night along his spine. This treatment was persevered in for six months, at the end of which he could walk pretty well without help, and could mount his pony without assistance. But still there was a dragging of his limbs and a tottering in his gait that were anything but satisfactory: and he complained at times of great pain in the spine, though now no longer confined to one spot. His bowels had also of late got very obstinate, and very often I had to pass the catheter for him, owing to a spasmodic closing of the urethra. Up to this time I had given my patient no strychnia, though I had been long wishing to try it; and had this been an hospital case I should have administered it long before. But here I was dealing with a person of rank; and before adopting any intended treatment, or "tampering with strychnia," as it would have been called, had my treatment made the patient worse, I had a consultation with Sir Henry Marsh, and with his approval

and consent I commenced administering strychnia to my patient, beginning with one-fortieth of a grain three times a day, gradually increased to *half a grain three times a day*. This enormous quantity he took for several days, at different periods, without suffering the least inconvenience. He then by equal degrees lessened the quantity retrogressively, till he returned to the *one-fortieth*, and then again increased it progressively to half a grain, and thus backwards and forwards for nearly a year and a half, at the end of which time he was in the enjoyment of perfect health, and continues so to the present day.

This was evidently not a case of softening, but one of undoubted hysteria. Had it been the former, no treatment could have been of any avail,—not even the old heroic bleeding and blistering system, once upon a time the sheet-anchor in almost every case, but now, fortunately, going fast into oblivion, and only lingering about the back surgeries of country apothecaries, who often are compelled to bleed their customers in self-defence, for if they did not, the customers would get themselves *blooded* elsewhere. It is perfectly astounding how this mania for bleeding, and being bled, exists in country parts. Not a month ago I saw, in an apothecary's shop, three poor fellows bleeding away at the same time, all in a row, greatly to their satisfaction, and to my unutterable disgust, for I could not find in any one of their cases the smallest excuse for venesection.

Many, however, are running to and fro, and knowledge is increasing. In every branch of learning *progress* is the watchword. The Marquis of Worcester's Century of Inventions has been completely outstripped in our day; for he never dreamed, in the wildest flights of his imagination, of the wonders accomplished in this age. We travel at the rate of a mile a minute; we make the lightning carry our messages, and the sun paint our pictures. Shall everything go *onwards*, and medicine alone remain stationary? No. So soon as physiology shall be *really studied* as anatomy is at present, and not *scampered* through in one short course, as is usual in the present curriculum of study; when the truths revealed by physiology shall be carefully applied by the practical physician,—then medicine also will progress, and indiscriminate bleeding and blistering fall into utter disuse.

The next disease I find in my note-book derives more interest from its rarity, perhaps, than from any peculiarity of treatment. It is one of Nyctalopia, and that not arising as an isolated case, but in the form of epidemic, a form which, though comparatively common in tropical climates, is almost unknown in these latitudes.

In the early part of 1852 we had an unusually long continuance of fine dry weather, and during a space of nearly eleven weeks we enjoyed a cloudless sky. Not a drop of rain fell during that period in this locality, with the exception of a slight shower, about the sixth week, not sufficient to lay the dust. I was at that time in charge of an auxiliary workhouse occupied solely by females, 117 in number, who had been drafted there from three several unions. They were of all ages, from the new-born babe of an hour to the drivelling hag of 90.

On the evening of the 17th of April I was summoned in all haste by the matron, who sent me word that all the women had *suddenly gone blind*. I at once went off, thinking all the while, in my own mind, that this was, in all probability, a case of "epidemic malingering;" never for a moment dreaming it could be genuine night blindness. On reaching the workhouse I went into the day-room, where all the women were assembled, and they all declared that *they could not see*. It was just dark, so I sent for a candle, and when on its arrival I began examining their eyes, I found every one, to my utter amazement, with dilated pupils. To my several questions they all gave the same answers. They had all previously the perfect use of their eyes in the unions from which they had been drafted, and they could see perfectly well till the sun went down that evening, when some quite suddenly, and others more gradually, became blind; the surrounding objects first became covered with a sort of haze, and then disappeared altogether. This was, then, no case of malingering, but genuine nyctalopia. I held a lighted candle so near to some of their eyes that they distinctly felt the heat of it, and yet the irides did not contract in the least, and the pupil retained the vacant stare of amaurosis. I had never seen even a single case of this disease in Ireland, and I was, therefore, much surprised at meeting it, for the first time, in an *epidemic* form. I had, however, seen it in the Mediterranean, and had remarked that it was always more prevalent during extremely bright glaring weather, and when, in consequence of the dazzling brilliancy of the moon, the nights were nearly as bright as the days (none who have only seen the moonlight in these countries can form an idea of its brilliancy in more southern latitudes). Remembering this, I commenced seeking for a cause, which, by acting alike upon all the inmates of the house, would be sufficient to account for the invasion of the present epidemic. I at once remarked that the dormitories were in a direction from north to south, having large windows in either side looking respectively east and west; these had no shutters nor blinds. The walls also were as white

as lime and water could make them. The yards, surrounded by high walls, were equally well whitewashed, and the ground was covered with clean white gravel: in short, everything in and about the place was painfully white, and not one single spot was to be found either of green, or any other colour, on which the eye could rest for a moment to relieve itself from the intense glare.

It appeared to me evident that this excessive amount of light was at the bottom of the mischief, and that the long continuance of clear bright weather, added to the intense whiteness of everything about the workhouse, had caused the appearance of this tropical disease. I therefore ordered black calico curtains to be at once made for the dormitories and day-rooms, both of the workhouse and infirmary; but, previous to having them put up, I determined to observe for myself on another evening, and to be at the workhouse at sunset, so as to witness the setting in and the progress of this strange complaint. On the following evening, a little before sunset, I was in the day-room of the workhouse; none of the inmates at this time complained of any defect of vision; I examined the eyes of several who on the previous evening had complained of total blindness, and found the irides reacting quite naturally to the light; but by degrees the sun went down, and then, as by magic, some quite suddenly, and some slowly, but *all* eventually, with very few exceptions, became totally amaurotic. The blinds which had been made were at once put up, and I gave directions that in the dormitories they should not be raised till the 6 o'clock bell rung in the morning, whilst those in the day-room were to be kept down in the east windows in the forenoon, and on the west windows in the afternoon; thus only to exclude the direct glare of the sun, without keeping up too darkened an atmosphere.

My conclusions proved correct, for on the very first evening after the use of the blinds *we had less amaurosis;* still, considerably more than half the number of inmates continued blind. In addition, therefore, to this physical treatment, I added a little smart purgation, by the administration of some compound powder of jalap, and on the following evening I was gratified by finding a still smaller number afflicted with blindness, and so on every evening less, till at the end of six days all had completely overcome the disease, with the exception of nineteen, who still became amaurotic every evening, notwithstanding the treatment which had been sufficient to cure the others. These I removed to the infirmary, placing them in a ward by themselves, and ordered them the following mix-

ture:—Sulphate of magnesia, two ounces; strychnia (in crystals), one grain; aromatic sulphuric acid, a drachm; and water, twenty fluid ounces: half an ounce to be taken morning and noon. A small blister, one inch in diameter, over each temple, close to the external angle of the eye; the blistered surface to be dressed morning and evening with an ointment containing three grains of strychnia to two ounces of lard.

Of these nineteen, five were completely cured by the end of one week, whilst the remaining fourteen continued just in the same condition, becoming amaurotic every evening immediately after sunset. To these I applied another blister, of the same size, on the forehead, over the external angle of the eye, which was dressed with the strychnine ointment, and at the same time I ordered them to continue the mixture they had been taking till then, adding one other half grain of strychnia to each twenty ounces of water. By the end of another week I had only six left in hospital, who were just as bad as when they were admitted. To these I applied two small blisters over each eyebrow, and dressed them with an ointment containing three grains of strychnia to the ounce. I persisted in this treatment for three weeks, the blisters being renewed as fast as they healed, commencing from the outer angle of the eye, over the brow, till they met in the centre of the forehead; and at the end of this period the last patient left the hospital perfectly cured.

Why some got well so quickly, while some were so refractory to treatment, I cannot pretend to say. I merely relate the facts as they happened. I think there can be no doubt that this disease was *caused* by the great glare of light to which the inmates of the workhouse were exposed; and as for the treatment, whether the strychnia cured the more stubborn cases, who can pretend to tell? I think it was of much service, and in a similar case I would certainly employ it again.

The next case in my note-book is headed "Atrophy." The patient was a young lady who had been ailing for many months. She had been seen by most of the leading physicians of Dublin, who, it appears, had not been able to diagnose much about her case, as the only name she brought back for her complaint was "atrophy." Most surely atrophied she was; I never witnessed greater emaciation. Notwithstanding, she ate a fair amount of food, and enjoyed it; she had no cough, no night-sweats; quite the contrary. Her skin was peculiarly dry and furfuraceous, as is generally the case in diabetes; so much so, that I diligently searched for sugar in her urine

(of which she voided considerable quantities), but without detecting even a trace of it. She slept well; had regular catamenia, though scanty: felt always extremely cold; suffered constantly from most obstinate constipation, and was in extremely low spirits. Such was her condition when she was brought to my house. After careful and minute examination, failing to detect any organic lesion, I came to the conclusion that it was a case of deficient nutrition, either depending on imperfect assimilation, or, if the food was assimilated, that there was occlusion of the thoracic duct, by some obstacle or other which I could not detect.

Every sort of nourishment had been tried with her, including *revalenta*. She had been drenched with cod-liver oil, and now refused to take any more. What was to be done? Food taken internally seemed to be actually thrown away. I determined to try what might be done endermically. I ordered her, therefore, to be well rubbed with cod-liver oil over the chest, abdomen, and back, every night, and to take a bitter aromatic infusion, with such a proportion of sulphate of strychnia that with every dose of the infusion she would receive one-fortieth of a grain of the latter three times a day. This plan, by inunction, was warmly adopted, and carried out by the patient, for she was anxious to be cured, and *for her* it had the additional merit of novelty. I shall not dawdle through a long account of her recovery; it was slow, but eventually she got perfectly well, and continues so to this day.

What merit can I claim for the strychnia in this case? Would not the oil have, perhaps, effected a cure by itself, whilst the strychnia by itself might have proved of no avail? Assuredly so; but I also firmly believe that the strychnia, by increasing the tone and the activity of the chylopoietic viscera, facilitated the assimilation of the food, while the additional nutrition effected through the pores of the skin was a most important assistant. But how can I prove that the strychnia had even the effect I claim for it? By the *immediate* cessation of the constipation; for whereas she would be often for two weeks without a motion, from the moment she commenced the strychnia her bowels acted regularly every day, without any artificial aid.

I had another case very similar to this, which I watched with the greatest care for more than three years; in which the greatest emaciation had suddenly succeeded to the most abundant *enbonpoint*. Here also I failed to detect any organic disease. The patient was a female of about 20; she had exactly the same symptoms as the previous case, and after lin-

gering on for three years, she died, and I unfortunately was unable to obtain permission to examine her body. I never could prevail on her to come into my infirmary; and, owing to circumstances, a regular system of treatment could not have been carried out in her own family; and, as I never could detect any organic lesion, the probabilities are, that in this case also, as in the previous one, I should have been successful.

In very many other forms of disease I have found the greatest benefit arise from the use of nux vomica and its preparations: in the incontinence of urine in the young, as also in that form which arises in females after long protracted labours; in some forms of atonic diarrhœa; and in that condition of the system where there is a tendency to furuncular inflammation, and the formation of anthrax. I am at present giving it in large doses, combined with quina, opium, and generous diet, to a poor lad of about 13, suffering from that dreadful form of periosteal inflammation and pyemia which, if I remember correctly, was so well described by the late Dr. M'Dowel of Dublin. And though the poor boy is dreadfully reduced from the number of incisions I was compelled to make, and the consequent discharge from so many suppurating surfaces, still, I do not despair of saving his life,—a result seldom obtained in such cases.

The last case of interest I shall notice is one of concussion of the spinal marrow. A man, more than fifty years of age, was following his trade of a slator upon a roof about twenty-five feet from the ground, when, by some accident, he slipped down the roof on his seat, and fell on a flagged space which immediately surrounded the building on which he was at work. He fell on his seat, apparently alighting on the point of the sacrum; and when he was raised up and carried into the house, his limbs were found to be completely paralyzed. I saw him about six hours after the accident, and he then had neither sensation nor power of motion in any of his limbs; his bladder was also paralyzed; his mental faculties unimpaired; he complained much of pain in his head; pulse full, hard, and quick. I had him carefully turned on his face, and I then examined him minutely, without finding any tender spot. I began by drawing off the urine, and then administered a full dose of croton-oil in a little brown sugar. The next morning early I found him much better as regarded his head, but complaining a good deal of an aching sensation down his spine. For this I ordered veratrine ointment, which gave him some relief, but the

passive condition of both limbs and bladder remained the same. On the morning of the fourth day after the acccident, I was called up to go to him as quickly as possible, as he was dying. I found him lying in bed on his back, *shouting from pain*, which he said was darting through his limbs, to such a degree of intensity that he could not control his screams. He was bathed in perspiration, with a full, soft pulse; he complained of no pain either in his head or spine; the spasms were entirely confined to his limbs, which, in addition to the pains, were also attacked by involuntary startings. His condition was so deplorable that I determined to give him chloroform, were it only to get him a temporary respite. He was easily brought under its influence, and I kept up its action, *without producing total insensibility*, for rather more than an hour. At the end of that time I stopped the inhalation, and having given him half an hour to fully recover from the effects of it, I administered a full dose of black drop, and left him. At my visit on the following day I was told that he had slept for many hours, and had been eventually awoke by a return of the pains and startings in his limbs, but these had not been nearly so violent as on the day before. His bowels being confined, I repeated the croton-oil in smaller quantity, and ordered a strip of belladonna plaster to be applied the whole length of his spine; this proved of the most signal benefit, and though for upwards of a fortnight he was subject to a return of the pains and startings, they invariably gave way to the application of the belladonna. One month after the receipt of the injury, his condition was as follows:—He could sit up in his chair, and move his limbs a little, but could not walk, and he complained of most distressing formication in them; he could not pass water; pulse regular; mind clear; bowels confined. He now began to take strychnia in doses of the one-fortieth of a grain three times a day, gradually increased to one quarter of a grain, and at the end of three months he was able to return to his trade.

I conclude, hoping that these cases, taken from "the Notebook of a Village Doctor," may induce many to give a fair trial to nux vomica and its preparations, and not to condemn and abandon its use *in disgust*, if at their first trial they do not find it to fulfil all their expectations.

Art. XVIII.—*On the Radical Cure of Reducible Inguinal Hernia.* By T. Spencer Wells, F. R. C. S., Lecturer on Surgery at the Grosvenor-place School, London; Surgeon to the Samaritan Hospital; Surgeon to the British Hospital at Smyrna, and Chief Surgeon to that at Renkioi during the late War.

In the thirty-seventh volume of the Medico-Chirurgical Transactions, published in 1854, there is a paper by me, entitled, "On the Radical Cure of Reducible Inguinal Hernia, by a New Operation; with Cases and Remarks." This paper was read on the 9th of May, 1854. In the Medical Times and Gazette of Jan. 23, 1858, there is a lecture on the same subject, delivered by me at the Grosvenor-place School. In the present communication I propose to give some account of the operations described in that paper and lecture, illustrating it by some additional woodcuts of the latest modifications of the instruments used.

By the term radical cure of inguinal hernia we imply such a closure or narrowing of the inguinal canal and rings, that none of the abdominal viscera can pass along the canal or through the rings. Attempts have been made to effect this closure or narrowing from the earliest ages of surgery. The application of the actual cautery to the abdominal ring, with the object of closing it by the granulation and cicatrization following the separation of the slough,—the use of caustics with the same object, caustic potash or lime, arsenic, sulphuric acid,—the ligature of the neck of the sac, either subcutaneously or after exposing it by incision of the integuments,—incision of the sac, followed or not by ligature of the neck,—excision of the sac,—castration, and ligature of the neck of the sac and cord together,—and various other measures, have been successively adopted, and abandoned as useless and dangerous. So that, at the present day, the balance of opinion among the most experienced surgeons of Great Britain, France, and Germany, is decidedly against any operation for the radical cure of hernia. And the opinion has been formed upon facts which fully justify it, for each and all of the proceedings above referred to have proved very fatal, have led to dangerous peritonitis, gangrene of the soft parts, or have been followed by a recurrence of the protrusion; so that the patient, even if he escape a great danger, has been left no better, and perhaps worse than before. These adhesions at the neck of the sac soon extend, become loose, and, giving way before the pressure of the viscera, a new hernial protrusion takes place.

But the question assumes a very different aspect if, discarding proceedings which experience teaches us to be useless or dangerous, we attempt the radical cure of hernia either (1) by inducing union of the two opposed serous surfaces of the hernial sac by exciting the exudation of lymph; or (2) by closing the rings and canal by inducing close union with an organic body pushed into the canal through the rings from without.

The adhesion of the opposed surfaces of the sac, and consequent obliteration of the cavity, have been attempted by subcutaneous scarification, by acupuncturation, and by the injection of iodine. This injection of iodine is alluded to by Mr. Fergusson, in his last edition, as a plan followed in America by some charlatans who attempt the radical cure of hernia by a secret method. Mr. Fergusson seems to think well of it, though it proved very unsuccessful in the practice of its originator, Velpeau. Indeed, any attempt merely to obliterate the sac, putting aside its great danger, is bad in principle, because the mere obliterated sac offers very little impediment to a new hernial protrusion, and all the methods which have been adopted with the hope of curing hernia on this principle have been abandoned, with the single exception of compression. A well-made truss, fitted accurately, if properly and permanently applied, gradually leads to exudation and adhesion. In this manner hernias of very considerable size are frequently radically cured in boys under the age of twelve years. But universal experience leads to the conclusion that, in adults, compression is so rarely followed by a radical cure, that a truss is only looked upon as a palliative, or a temporary safeguard.

Various methods have been devised for carrying out the second principle. Some have proved unsuccessful, but others, in which a portion of the skin of the patient's scrotum is used to close the inguinal canal, have been more fortunate. One of the earliest and simplest of these plans was devised by Gerdy, and practised many years ago at Guy's Hospital by Mr. Bransby Cooper. Gerdy was fully aware of the necessity, that the adhesive inflammation he desired to excite in the hernial sac should not extend to the general peritoneal cavity; but he fell into the mistake of supposing that he could push a portion of scrotum into the inguinal canal, and fix it there by a needle and thread passed from within this portion of scrotum—this plug—outwards through the abdominal parietes, without perforating the sac. It may be possible to do this occasionally, but, as a general rule, the sac is not pushed before the invaginated scrotum. In some exceptional cases, where the patient has not worn a truss, the sac may be pushed completely into the abdomen; and

in some cases of old hernia, where the sac is thick, and the surrounding cellular tissue very loose, the same may be done, the sac hanging into the abdomen from the internal ring like an empty purse. But these are comparatively rare cases. In the great majority, even in recent cases, the serous layer of the hernial sac is so firmly adherent to the walls of the inguinal canal, that it can only be separated by dissection. Let this

Fig. A.

Fig. B.

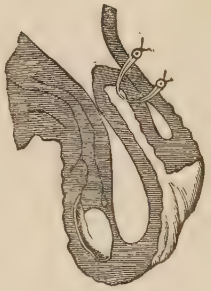
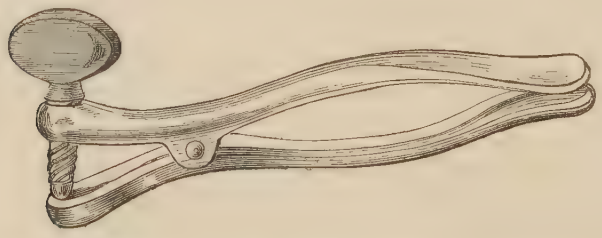


Fig. C.

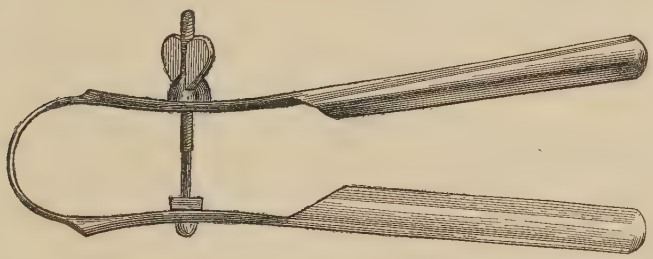
diagram (Fig. A) represent the sac and abdomen both empty; the peritoneum and its prolongation, forming the lining membrane of the canal—the sac—are of course continuous. Now suppose the canal be plugged by pushing a portion of scrotum into it in this manner, the result will be what is shown in the annexed diagram (Fig. B) of what I believe to be the effect of Gerdy's operation. The plug is fastened in its new position by a suture, and the result is union of the opposed surfaces of the sac over a very small space. Such slight adhesion would appear likely to give way before very moderate pressure, and experience proved this to be the case, for relapses are very common after Gerdy's operation (Fig. C).

A more perfect closure of the canal was, therefore, evidently



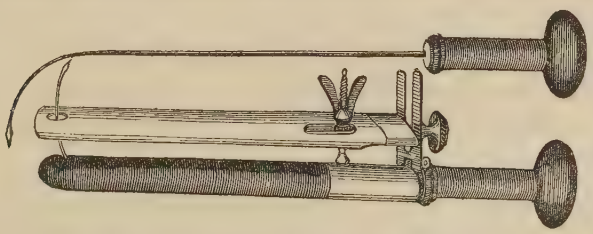
desirable, and various plans were devised for effecting this closure. Schuh, of Vienna, used the thread of Gerdy for draw-

ing up into the canal a short wick of cotton, and counter-pressure was made by a compress and bandage, but this did not succeed to his satisfaction. The pressure was neither firm nor uniform. Max. Langenbeck contrived another instrument (figured at p. 350) which he used with some success, but it was designedly left so long applied that sloughing of the compressed parts followed, and it was to the subsequent granulation that Langenbeck looked for closure of the canal. The process required long confinement to bed, and was altogether too severe to come into general use. Leroy D'Etiolles contrived another instrument. It is a metallic cylinder and a cover, which can be so screwed together as to press the invaginated scrotum and abdominal parietes together. If this could be fastened tight enough to prevent its slipping out without injuring the compressed skin, it might be a useful instrument, but it cannot. Mr. Fergusson, the instrument-maker of St. Bartholomew's Hospital, has been trying to improve this instrument at my suggestion.



The screw seen in the woodcut is brought nearer the cylinder, and a needle screw passes through both cover and cylinder from without, near the extremity. Both cover and cylinder are made of wood, but I think the instrument, even in this state, is inferior to Wutzer's.

Wutzer was Professor of Clinical Surgery at Bonn. He still lives there, but has retired from his Chair. His instrument,



made by his own maker at Bonn, the one I used in my early operation, is shown in the annexed diagram. Below is seen

the entire instrument, while above there is a second view of the needle, which passes through its centre when withdrawn.

It may be seen that the instrument consists first of a cylinder of very hard wood. This is made of different lengths and diameters, according to the breadth of the canal. It is destined to take the place of the index finger, after the latter has pushed a part of the scrotum through the abdominal ring into the inguinal canal. Towards its anterior blunt extremity it becomes gradually thinner. It contains a canal, lined with metal, which conducts an elastic steel needle, flattened on the point, and furnished with a movable handle. A round opening near the point of the cylinder allows the needle to pass through, so that, when the cylinder has been properly introduced, pressure upon the handle of the needle sends its point along the interior of the cylinder, the skin of the scrotum, the serous coat and coverings of the hernial sac, projecting at last through the integuments. In order to increase the pressure which the wooden body remaining in the canal itself exercises, a movable case of hard wood is made concave, corresponding to the outer convex side of the cylinder. It is made rather wider than the cylinder, projecting two or three lines on either side, in order to distribute the pressure more equally, and near the end is an opening to receive the projecting point of the needle, which thus fixes one end of the cover over the cylinder. The other end is supported upon a movable metallic staff; near this is a screw, by means of which cover and cylinder can be pressed together to any degree of strength, so that in a moment the anterior wall of the hernial sac, of the inguinal canal, and the tissues between the cylinder and the cover, can be compressed to the precise degree each case may require.

The cylinders are made of various calibers, to adapt them to the different diameters of the inguinal canal, as a great deal depends on the proper filling of the canal by the cylinder, the pressure of which should operate as equally as possible upon all parts of the inner suface of the sac. On the other hand, the diameter of the cylinder must not be too great, or it would be impossible to pass this blunt end to the internal ring, and our object would be defeated; a diameter of five to seven lines suits most cases. The invaginated scrotum fills the rest of the canal.

After using this instrument of Wutzer's, I soon found certain alterations desirable. The needle got rusty, caused undue irritation at the point of puncture, and became unfit for use again. This was soon obviated by electro-plating it. Then the necessity for varying the size of the cylinder in each case

was troublesome and expensive. I obviated this, though imperfectly, by rolling strips of adhesive plaster round the cylinder until I made it as large as I desired. But this led to irregular pressure, and in a case I saw with Mr. Stretton, now of Wolverhampton, caused a little sloughing, which, though it did no harm might as well have been avoided. The cylinder, again, was too round for the normal shape of the canal. The point of the needle had to be guarded by a piece of cork, which would perversely be knocked off sometimes, and, worse than all, there was nothing to prevent the needle itself from slipping back—the only thing keeping cylinder and cover in their place. I had thought of different plans for correcting these faults, but not of anything so good as those for which we are indebted to Professor Rothmund of Munich. His instrument is represented in the accompanying cut. It shows the instrument complete, and its different parts screwed together. It is

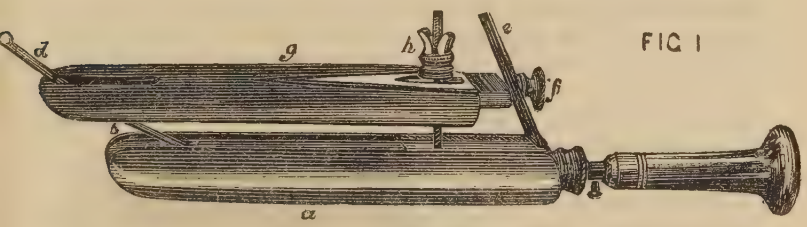


FIG 1

made precisely on the same principle as that of Wutzer, and at first sight resembles it exactly in appearance, but the cylinder is oval, not round, indeed it is rather too oval, or flat, and I now find that a form flatter than Wutzer's, but rounder than Rothmund's, is the most generally useful. The needle is not steel, but silver, with a movable steel point, and there is a knob which can be screwed on after the point is removed, and at the handle there is a spring which is an effectual safeguard against the needle slipping from its place. Lastly, in order to admit of the same cylinder serving for the occlusion of canals of various dimensions, it is so made that side-pieces of different sizes can be fitted on to the central portion of the cylinder. There are covers of different sizes, so that by altering the side-pieces we can make the cylinder quite as large as one needle could fix well. In case of very large rings and canals it is necessary to have the central piece perforated for two, or even three needles.

By this instrument we can alter the dimensions of the cylinder, so that, while perfectly filling the canal, it may push the

plug of the invaginated scrotum before it well up to the internal ring, and even project some little distance into the abdomen. We can fix the plug much more securely and evenly than by a suture, or either of the other compressing instruments, and we have complete command over the degree of pressure exercised.

The annexed cut shows the cylinder with three needles

FIG 2

passed though the central and side-pieces; on one needle the point remains, on another the point is removed, and on the third the knob is fitted. The cuts show the cover for the instrument with one and with three needles. I have not thought it necessary to have drawings made of the instrument and cover with two needles, as it is one simply larger than the single, and smaller than the triple instrument. The last engraving shows the needle withdrawn, the spring near the handle which keeps the needle in its place when pushed home, and one of the side-pieces removed from the central por-

FIG 3

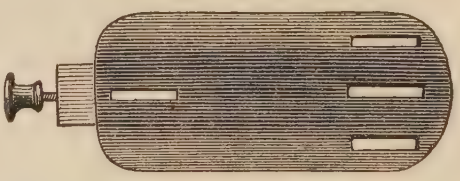


FIG 4

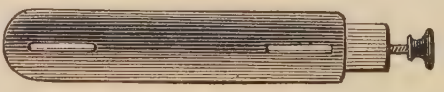
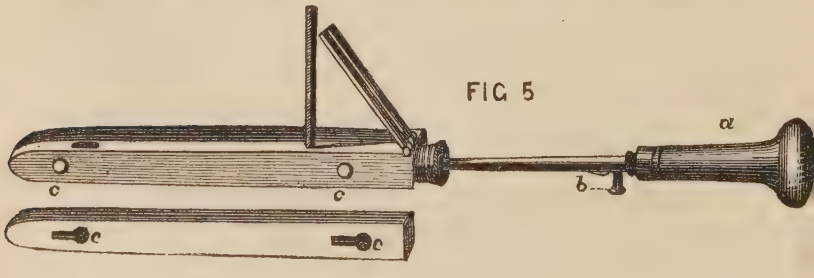


FIG 5

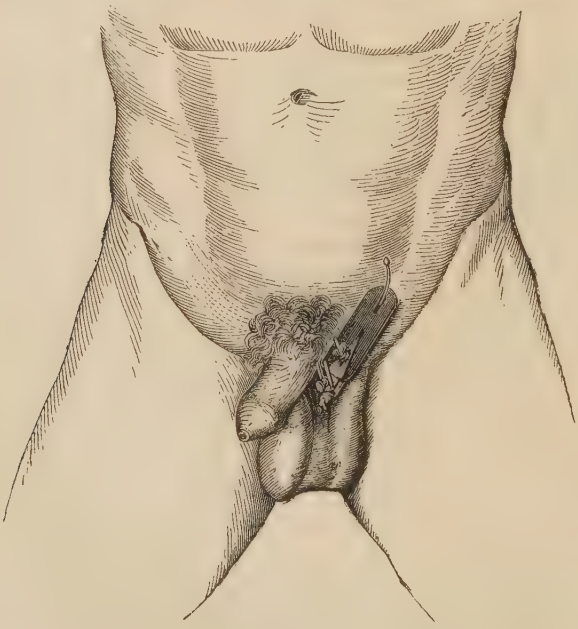


tion of the cylinder, with the openings in the side-piece into which projecting buttons on the cylinder are received.

The manner of using these instruments of Wutzer and Rothmund is the same. The preparatory and after treatment is the same. The patient's bowels should be opened a day or two before the operation, and the rectum cleared the same day by an enema of warm water. The hair is removed from the side affected, the bladder emptied, and the hernia carefully reduced. This being done, the patient is placed on his back with the shoulders raised, the thighs semiflexed, and the knees separated, just as in reducing a hernia by the taxis. The surgeon stands on the side to be operated on. Perhaps I may be here allowed to quote from my lecture the following account of the operation, as the familiar mode adopted in lecturing may be clearer than a more formal description:—

"If you can use the right and left hand equally well, you may use the left forefinger for invaginating on the left side, and the right to hold the instrument—the reverse for the right side. But the left finger will do for invaginating on either side very well. You see I operate on the left side—so I stand on the left side of the patient. I place my left forefinger here on the scrotum, about an inch below the external ring, and then push a fold of the scrotum before my finger with a little rotatory movement slowly and steadily into the canal, keeping the palmar surface of the finger turned forwards and a little outwards, until it is well under the tendon of the external oblique, and the plug of scrotum is well pressed up to, or through, the internal ring. If you place one forefinger on the abdomen, just over the internal ring, while the other is in the canal beneath the tendon of the external oblique, you will feel the tendon very distinctly. As you move the finger backwards and forwards, it rolls over the tendon. You should accustom yourselves to feel this, because it is the test by which you know and can be certain that you are well in the canal, and, when you have introduced the cylinder, that it is in the canal, and has not slipped anterior to it. When you are quite sure that your finger is in the canal, the next thing is to get the cylinder into the place of the finger, and then to fix it there by the needle. To do this you take the cylinder with the needle passed within it as far as you can without permitting the point to project, and hold it as I do now in the right hand with the thumb before the screw, the forefinger and second finger on either side of the prong which supports the cover, and the ring and little finger on the under side of the cylinder. Then bend the left forefinger a little, draw it forwards,

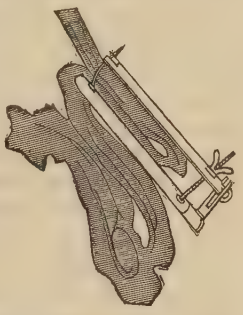
and slip the cylinder along its dorsal aspect at the same time as the finger itself is being withdrawn. This is the most important step of the whole operation, and the only step which is at all difficult. Without care the plug may follow the finger. Without care the cylinder, instead of slipping beneath the tendon of the external oblique, may slip between it and the integument. You must be very careful that this does not occur, by feeling, as you felt when your finger was in the canal, that the tendon rolls over the cylinder. If you do not feel this, and if the cylinder moves freely beneath the integument, you may be quite sure it is not in the canal, and you must begin again. I wish particularly to guard against this mistake, because I *know* it has been made once, and I *suspect* oftener. If you feel the tendon rolling over the cylinder, you may be quite sure it is in the canal, and the end well up to the internal ring. To fix it there I have only to push on the needle till its point appears through the abdominal parietes, then to put on the cover, and use the horizontal and perpendicular screws until the cover and cylinder are evenly pressed



together. Then the point of the needle is unscrewed, the knob put on, the handle of the needle removed, and all is

done. You leave the patient with the instrument secured thus. The whole affair does not take a minute, and as the only part of it at all painful is the simple puncture of the needle, it is quite unnecessary to give chloroform.

"This diagram shows what I believe to be the real action of the instrument. Here you see how plug, sac, and integument are fixed and pressed together. In some few cases the sac may be pushed up before the cylinder; in some only a very small portion may be below the needle; and in others it may extend throughout the whole line of compression. If the cylinder fill the canal well, the pressure is quite sufficient to cause adhesion behind as well as before the cylinder. Some, to insure this, have kept up pressure on the cylinder by a compress and bandage; but this is unnecessary, and even injurious, as it leads to inflammation of the spermatic cord.



"I should have told you that you should oil the cylinder before using it, or else smear it, as Rothmund does, with cantharides ointment. Wutzer does not think it at all necessary to remove the epidermis of the invaginated scrotum. He looks upon the adhesion of the sac as the really important part of the operation. Rothmund, however, thinks additional security is obtained by removing the epidermis, and thus procuring adhesion of the opposed surfaces of the plug. He thinks also the irritation of the skin set up by the cantharides hastens the adhesive process in the sac, so that the instrument may be removed as soon as the fifth day, instead of leaving it to the seventh or eighth. He speaks from such very large experience that I feel inclined to follow his practice for the future, although I am sure it is not at all *necessary* to success.

"Then as to the degree of pressure you apply with the screw upon the cover, it should be very slight the first day, to allow for a little swelling. The next day the screw may be tightened or loosened according to the state of the skin. The patient should be kept in bed, lying on his back, with the knees raised by a pillow, and the scrotum supported by a cushion or folded towel or two put between the legs. Every day the cover should be raised, to see whether the pressure is equal, and not too great. If it does not press evenly, a little cotton wool may be inserted; or if it press too much at any point, this may be raised by a little cotton put near it.

"About the fourth or fifth day you see a little inflammatory redness and swelling round the needle puncture, and commencing suppuration. On the sixth or seventh some serous fluid begins to escape round the end of the cylinder. This shows that the epidermis is separating. There is more purulent discharge from the puncture, and the instrument may then be taken away by withdrawing the needle and carefully removing the cylinder. You will find that the skin of the scrotum may be pulled tolerably hard without yielding at all, showing that pretty firm adhesion has taken place. If it appear to yield on pulling slightly, it would be well to replace the instrument for two or three days longer.

"The after treatment will vary a little according to your desire to produce adhesion of the opposed surfaces of the plug to each other or not. Wutzer filled the cavity left by the withdrawal of the cylinder with charpie, dressed the puncture simply, and then put on a common bandage. Rothmund endeavours to obliterate the cavity. He keeps the patient in bed, raises the scrotum by a suspensory bandage, keeps off the pressure of the bed-clothes by a cradle, applies simple dressing to the puncture, and over this a graduated compress which is made to exert some pressure and keep the two sides of the cavity together by a common figure of 8 bandage. The bandage is changed as often as it becomes dirtied by the secretion, and is carefully reapplied. It requires from twelve to fifteen days to secure obliteration of the cavity and cicatrization of the puncture. As I said before, it is only Rothmund's great experience which would lead me to think much of the importance of this obliteration. I tried it in my second case, but no union took place, and in cases where union has taken place, the condition of parts a few months after the operation has been precisely similar to those in which Wutzer's practice was followed. Perhaps a middle course will eventually be adopted, not filling the cavity with charpie, nor keeping the patient to the bed or sofa after cicatrization of the puncture, but keeping him quiet until this cicatrization is firm, and leaving the cavity to close, if it will, under the pressure of a compress and bandage, but not being sufficiently anxious about it to prolong the confinement of the patient. In either case the inguinal canal is filled by a firm plug, and for some weeks a sort of puckered depression is seen at its mouth, but this disappears after a few months, although the plug may still be felt. After five or six years the plug itself can scarcely be felt, and the most careful examination can hardly detect anything abnormal. Rothmund convinced himself that this

could not be explained by the sinking of the invaginated scrotal plug to its former position, by tatooing round the opening of the cavity, and observing that the marks did not change their position as they would have done had the plug descended.

"If the pressure has been unequal, or the instrument left a day or two too long, there may be a little sloughing around the needle; but I have never seen a slough larger than a sixpence, and this only once. The only ill effect of it is some delay in the cicatrization. I believe if there is ever more sloughing than this, it must be from carelessness of the surgeon, or some unfortunate condition of the patient, or the crowding and defective ventilation of some hospital ward.

"Allowing six or seven days for the period the instrument is applied, and from seven to fourteen days more till cicatrization is complete, you should generally prepare a patient to expect a confinement of three weeks to his room. After this, he must wear for two or three months a very elastic truss, with a weak spring and large well-stuffed pad. If the pressure is too great, or the pad too small, absorption of the plug may take place, and relapse of the hernia follow. If no truss be worn, the adhesions which are still soft and yielding might give way. It is also well to have a suspensory bandage worn as long as the truss, that the weight of the testicles may not drag down the skin of the scrotum to its former position. You should also forbid violent exercise until the truss can be left off. After three months the truss may be left off, and I believe the patient is fully as secure against the occurrence of hernia on the side operated on as on the other side, perhaps more so; in other words, that he has not only been cured radically of his hernia, but he has been protected against its occurrence—he is less likely to become subject to hernia on that side than a healthy man."

We are still in want of knowledge of the precise anatomical conditions resulting from this operation, which post-mortem examination alone can afford. One of my patients died of phthisis while I was abroad, so that the opportunity was lost in that case. Rothmund has never in all his cases had one fatal result, although he had several when he operated by the methods of Gerdy, Bonnet, and Belmas, nor has he happened to be able to open one of his patients who have died of some other disease; but Dr. Thormann, of Chur, opened a patient of Rothmund's, who died a *short time*—the precise time is not given—after the operation, of some other affection, and he has described the appearance. The patient, twenty years old, of middle size,

suffered from a reducible oblique inguinal hernia, of the size of a hen's egg, on the left side. The canal and rings were small, and the smallest cylinder was used. The radical cure was effected; and when the patient died of some other disease, Dr. Thormann found, after death, the invaginated scrotal plug so firmly united by adhesive inflammation to all parts of the canal, that it could not be separated without dissection. The external ring and the whole canal, to within six lines of the internal ring, were completely filled and closed up by the adhesion of the plug. This is the only account of a post-mortem examination after Wutzer's operation that I have been able to find.

Now as to the safety and success of this operation, it has been performed about fifty times by British surgeons. I assisted Dr. Burmester to do it in Malta in 1847. I did it myself in Malta in 1848, and in England in 1850, after a visit I had paid to Wutzer in Bonn, when I saw some of his cases and procured his instruments. Between May, 1854, and February, 1855, I did four other cases in England. I then went to the East, and Mr. Stretton, one of the surgeons in my division of the hospital at Renkioi, did it upon a Greek. Mr. Holmes Coote was there at the time, and saw this case. He was so pleased with the result that on his return to England he performed the operation at St. Bartholomew's Hospital, and thereby deserves the credit of introducing it into London hospital practice. This was more than two years after my paper had been read at the Medico-Chirurgical Society,—a curious example of the slow progress practical improvements make in the large hospitals of the metropolis. Then Mr. Jones, of Jersey, took up the operation, and as he asked me to get the instruments made for him by Mr. Coxeter, I directed Mr. Coxeter to have the needle silvered, to obviate the rusting, which I had found objectionable. I think Dr. Vaudin, of Jersey, and Mr. Hutchinson, were the next to take up the practice, and then Mr. Erichsen. More recently, Mr. Cumming, of Exeter, Mr. Bickersteth, of Liverpool, Mr. Holthouse, and Mr. Tamplin, have done it. I have had six more cases since my return from the East, and these are all that I have heard of in this country. If we add Dr. Burmester's, Mr. Stretton's, and my own case done abroad, to the eleven of my own, seven of Mr. Jones', three of Dr. Vaudin's, three of Mr. Coote's, two of Mr. Hutchinson's, four of Mr. Erichsen's, and one of Mr. Cumming's, three of Mr. Holt, Mr. Tamplin, and Mr. Bickersteth, done in England,—we find that of these there has not been a death or dangerous symptom in any one case. But, more than this, the operation has been almost uniformly successful. I do not lay great stress

on the cases operated on within the last year, although they assist in proving the safety of the operation, as they are still open to the possibility of relapse; but some of my own cases have now passed ten, nine, seven, and three years, without a sign of return. I have not had a relapse, or a bad symptom, in any one case. Mr. Jones tells me the same thing. He says he has not seen anything to give him the slightest fear or anxiety after the operation; and its success has been complete. He did it in one person as old as sixty-three. He told me of one man whom he saw drunk in a wine-shop in Jersey a few weeks after the operation, wearing no truss, and jumping on and off the counter to show how well he had been cured. Mr. Coote's three cases were all successful, but he tells me that in another case "a man was persuaded against his will to undergo the operation. The needle was too thin, and it bent, and missed the border of the ring, consequently the plug of integument slipped out. The patient had no bad symptom, but refused to have the operation completed." Mr. Erichsen informs me that of his four cases three succeeded, but "in the fourth it failed in consequence of the patient, a lad, walking about the ward while the instrument was still applied." One of Mr. Hutchinson's cases succeeded; in the other he tells me the man had a very large hernia, and suffered from violent cough. Symptoms of inflammation of the sac—not peritonitis—came on, which induced him to remove the instrument on the fourth day. The man did well, but the hernia was not radically cured.

A case related to me by Dr. Vaudin, of Jersey, tested the value of the operation most completely. A man, thirty years of age, the subject of phthisis, had a large oblique inguinal hernia for seven years. The operation was performed on the 6th of October last. The man died about the 1st of January, but, notwithstanding the severe coughing, there was no descent of the hernia. Unfortunately, a post-mortem was not permitted. Dr. Vaudin adds:—"I have not heard one of the patients operated on complain of pain or discomfort caused by the operation." In another of his cases the operation was severely tested by a two-months' pedestrian tour, but no relapse has occurred. Two of Mr. Jones' patients were seamen, who are now following their occupation without wearing trusses. Some of my own patients are men of very active habits, and I have not yet seen a sign of recurrence of protrusion. The only case of failure I have had was a patient at Liverpool, upon whom I operated last February. The patient was old, the hernia very large, the rings widely dilated, and the canal almost obliterated. There was a violent chronic cough, and no truss sufficed to keep up

the hernia. Thus the case was a most unpromising one, and I did not hope for more than such a narrowing of the ring as would render a truss effectual. I was obliged to return to town on the day of the operation, but Mr. Leathem, a very able and experienced surgeon, attended carefully to the case. He tells me that he kept up firm pressure by the screw, but there never was a sufficient degree of inflammation excited to produce adhesion. No harm was done, but the case was a failure, and the first I have met with.

I think I may say, therefore, that the experience of British surgeons, so far as it has gone, speaks strongly in favour of the operation, and if we look to the Continent we find a much larger store of recorded experience. Wutzer, in a letter to me, in 1853, says:—"I am not able at present to give you the statistical results of all the cases upon which I have operated, as I have not time to collate them. I can now only say that, since the autumn of 1838, I have repeatedly practised my operation in the clinique every session before many witnesses, and that I have never seen severe peritonitis follow it, still less any fatal result. All those operated on have not been cured. In several, relapse followed, but this was traceable either to the patient leaving off the truss too soon, or undertaking very hard bodily labour soon after the operation." Professor Sigmund, of Vienna, informed me that he had performed the operation nineteen times in the great Hospital of that city, with complete success in fifteen cases. No death happened, but in two cases there was some gangrene of the integuments, and in two relapse occurred. I have heard that death did follow in one case at Brussels, but that the operation was very unskilfully performed upon a patient suffering at the time from primary syphilis; so that this misfortune is not to be attributed to the operation, but to the operator.

Last autumn I was in Munich, and I called on Professor Rothmund, who has a larger experience of the operation than any other surgeon. I saw him perform the operation in one case, and saw two men upon whom he had operated, on one three days, and on the other ten days before. I had a long conversation both with the Professor and his assistants. They said the increasing number of patients coming for operation was the best proof of its safety and success. At first Rothmund found it difficult to induce ten patients to submit to it in a year; now they apply every day. They are mostly working men; and one tells the other how he has been cured. On inquiry as to relapse, the answer confirmed my previous impressions, namely, that in cases where the

canal is not larger than to admit the finger easily, the radical cure may be relied on with almost absolute certainty, and that the probability of relapse increases with the size of the canal and rings. When the rings are very large and the canal very short, the chief use of the operation is to make a truss effective. In these cases of widely dilated rings and short canal my impression was that the operation might be hazardous, but the experience at Munich shows that this fear is unfounded, provided very large cylinders be used, so that the rings are well filled, and descent of intestine by the side of the cylinder is impossible. Rothmund has done the operation about 400 times in the Clinical Hospital at Munich, and he told me that he had done it much more frequently in private practice, so that he felt convinced he must have operated a thousand times, and without one fatal result. This, I think, is more than almost any one could say of the most trifling operation. No one could expect to do a thousand operations for hare-lip—to tie a thousand nævi or piles—to remove a thousand small tumours —in fact, do the most trivial surgical operation a thousand times, without some untoward result following accidentally, in some one case. So that this evidence is very strong indeed in favour of the almost perfect safety of the proceeding.

The only objection to the operation it appears at all necessary to reply to is the very common one that we want no radical cure for hernia; that the palliative treatment is sufficient; that a good truss is far better than any radical cure. There is some plausibility here, but it must be admitted that the fatality after operations for strangulated hernia is very great, and that a large proportion of those who become the subjects for these operations have worn trusses. The trusses have not proved a safeguard against strangulation; nay, by causing thickening of the sac, and contraction of the hernial apertures, they have increased the danger of strangulation when it has taken place. Then all trusses are not good, and good trusses are expensive. They get out of order, and patients get careless and do not keep them in order; and even when good and in order they are a very insecure safeguard in sudden and violent motions of the body, or in cases of long-continued exertion. Besides, those who wear trusses say that it is not pleasant to wear them all their lives, and carry about with them a constant memento of bodily imperfection and possible danger. Contrast this with the three weeks' confinement to the house, and the subsequent safety obtained by the operation I advocate, and I trust the profession in Ireland will assist in hastening the general recognition of its value.

The relief of a strangulated hernia is justly regarded as one of the noblest triumphs of operative surgery. The surgeon saves the life of the patient without removing or deforming any part of the body. But the surgeon who cures hernia radically, with certainty and safety, is a greater public benefactor, as he not only relieves large numbers of his fellow-creatures from suffering, and the inconvenience of wearing a truss, but he averts the danger of strangulation to which they are continually exposed, in a greater or less degree, through every period of life.

With regard to the class of cases in which the operation I have described is indicated, I may be permitted to quote a passage from my paper in the Medico-Chirurgical Transactions in 1854:—

"As to the class of cases in which it is indicated, perhaps we might say—1st. In all strong, otherwise healthy adults, up to forty or forty-five years of age, who lead a life of active bodily exercise. In such patients, where the hernia has only acquired a moderate size, has not become adherent, and where the long diameter of the inguinal canal has not been much shortened by the continued pressure of the intestine, we can most certainly depend upon the excitement of a passive exudative inflammation with subsequent adhesion—in other words, upon a radical cure.

"The inguinal canal becomes wider, and at the same time shorter and narrower, the more extensive, old, and neglected the hernia may be. At length the shortness of the canal, the relaxation of its walls, and the large circumference of both rings, form so many impediments to a successful union; and the attempt at a radical cure will be more unsafe in proportion as these impediments increase. All this must be taken into account when considering the second class of cases in which this operation may become advisable, namely, in patients who have not arrived at the age of decrepitude, whose hernial tumours cannot be longer kept up by any mechanical assistance. This may be the case when the inguinal canal is extremely dilated and shortened; in certain species of omental hernia; in cases of great sensibility of the spermatic cord; and in persons with a fat, pendulous abdomen, upon whom the pad of the truss slips when they move. Such persons live in constant danger of strangulation; and in them herniotomy would be infinitely more dangerous than invagination, performed at a proper time. If the neglect have been carried to its highest degree, and the rings have become widely dilated, probably no method of operation will avail to close them per-

fectly; yet the patient gains a great deal if, by such a proceeding, the evil can be at least so much diminished that a truss may be successfully worn. In one such case, operated on by Professor Rothmund, in an old woman who had a labial hernia which had reached the knee, four fingers together could be passed through the ring, and no truss was of any use; yet, although no radical cure was effected by the operation, it had the good effect that the hernia could be afterwards properly kept up by a truss."

Since that paper was written I have had a case in a man fifty-nine years of age; and Mr. Jones, of Jersey, in one of sixty-three; while, on the other hand, Mr. Bickersteth has operated on a boy nine years of age, and Mr. Tamplin on a child only two years old. All these cases are successful. My own feeling has hitherto been against any operation in children, as compression by a truss so often effects a radical cure; but these cases of Mr. Bickersteth and Mr. Tamplin induce one to inquire whether it may not be better to operate even on young children, than to expose them for several years to the inconvenience of a truss, with the probability that, after all, a radical cure may not be obtained.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Cause of the Coagulation of the Blood: being the Astley Cooper Prize Essay for 1856, *with Additional Observations and Experiments: and with an Appendix, showing the Bearings of the Subject on Practical Medicine and Pathology.* By Benjamin Ward Richardson, M.D., Licentiate of the Royal College of Physicians of London, &c. &c. London: Churchill. 1858. 8vo, pp. 466.

Not many centuries have elapsed since the range of science assumed one undeviating path, and mankind beheld the most enlightened of their species patiently devoting time, energy, midnight toil and vigil, to the unwearied pursuit after an object as chimerical in its existence as it must have become valueless in its results, had they succeeded in their undertaking, and had the mode of insuring that success become, as it must necessarily have done, generally known. The love of life, inherently implanted in our existence from the moment of our birth—a feeling acquiring additional strength with each progressive year of our growth, fostered and cherished by fresh ties developed in our manhood, acknowledging no diminution in its intensity with the declining years of ripe old age—in former times led the sage and the philosopher to devote the whole of those talents with which a bounteous Providence had endowed them, to the vain and futile attempt of endeavouring to wring from nature that impenetrable secret so wisely hidden from the eye of our prying curiosity, by which they might be enabled to check and avert that gradual decay to which all earthly things have been destined from the beginning, and to substitute in lieu of it a perpetual adolescence, over which the hand of death could exercise no dominion. A pursuit so congenial to the wishes of the multitude could not fail of obtaining numerous

votaries; and we shall accordingly find that fortunes were prodigally lavished in the effort to reduce to practice a host of insane theories, which as continually failed, only to be again renewed and repeated with a pertinacity and perseverance worthy, in any other cause, of a far different fate. Besides, the age was remarkable for its credulity, of which a mass of pretenders to science took immediate advantage, preying with an audacity the most unparalleled on the resources of their richer brethren; while those latter, untaught by experience, with an infatuation that is scarcely credible, allowed themselves to be made the willing dupes of a designing race, awakening only at length to a proper perception of their error, when absolute incapacity to furnish further means from their exhausted resources too truly told the tale of their utter destitution and ruin.

In the present advanced stage of education, we find it difficult to repress a smile at such a gross display of combined weakness and simplicity on the part of our forefathers; but we should not, at the same time, altogether overlook the fact that there were only few, if indeed any, counteracting influences at work to modify or control the principles that predominated so completely at the period to which we allude. The merest germs of rudimentary knowledge were strictly confined to the few alone, and these, unfortunately, were more apt to employ that knowledge, trivial as it was, in purposes immediately connected with their own personal aggrandisement, than to devote their time and acquirements to subjects adapted to promote and improve the general well-being of society. Some, indeed, there were, who, actuated by higher and purer motives, endeavoured, by their writings and reasonings, to inculcate doctrines of a nobler and more truly philosophic character; but such was the spirit of the age in which they lived, such its perversity, and such the superstitious folly with which they had to contend, that all their efforts were abortive; for the infatuated multitude would turn in disgust from the wisdom of their teachings, to listen with unqualified delight to the rampant ravings of a cunning and audacious charlatan. Nor was it natural that any other result could possibly be anticipated in the midst of such a chaos of ignorance as was then so generally prevalent, more especially where a ready means of propagating ideas was comparatively unknown, or at best confined to a very narrow circle. Sentiments tending to exalt, elevate, and enoble the human mind, cannot be expected to take root and flourish in a coarse and sterile soil; and the hand of cultivation must be carefully applied before such seeds will germinate, arrive at a happy maturity, and produce their habitual fruits.

But with the discovery of the art of printing, and the more general diffusion of knowledge—its natural consequence—circumstances became wonderfully altered; and a little reflection must satisfy us, that out of two of the most monstrous hallucinations that ever seriously engaged the attention of mankind, originated much of our information—at least on the fundamental parts—of two of the highest sciences of modern times: we allude to chemistry and physiology. In the one instance, the attempt at the transmutation of metals, compelling, if we might so express it, experiments in a continued series, and all of a different character, was most undoubtedly the cause of many valuable and important discoveries in this peculiar branch; and it would appear that no amount of failure was sufficient to discourage the impassioned votaries of this art from renewed efforts in their favourite pursuit, but rather seemed to stimulate their energies to fresh trials, the prospect of ultimate success inciting them to redoubled exertion on their parts. From the records of the past we may learn that their self-imposed avocation was attended not only with much toil, but also with no small degree of personal danger, for the properties of bodies were then but imperfectly, if at all, understood, and their combinations were accordingly sometimes productive of results totally unexpected, and too often such as were calculated to imperil the very existence of the unwary experimenter. Still, as fact after fact became gradually discovered, and as gradually propagated, amongst this class of visionary inquirers, a perfect chain of axioms was at length happily established, generally indeed subversive of, or, at all events, antagonistic to their favourite preconceived opinions; but every one of them tending to one great end—expansion of the mind—stripping it most effectually of those narrow prejudices that grew up spontaneously, and in complete unison with the false and groundless views of a dark and unenlightened age, and engendering in their place a taste for a pursuit more rational, and more in consonance with man's exalted intelligence.

The very idea of endeavouring to transmute a comparatively worthless material into another by general consent alone, allowed to be of greater value, though intrinsically possessing no claim to such a marked distinction, clearly implies a cupidity for the latter commodity,—and hence originates that love of wealth so general amongst the mass of mankind. But still it is a principle by no means innate, or originally implanted in our nature at birth, and can be only regarded as acquired, deriving its force and intensity from no other cause but observation, pointing out to the eye of every beholder the enormous

mass of influence and power that by its agency is invariably thrown into the hands of its fortunate possessor; and hence the earnest zeal that is displayed for its accumulation. With the principle of life, however, the case is widely different; for, from the lowest creature in the scale of creation to the very highest in the range of vitality, we shall find that a love for it is most intimately allied from the first hours of existence; that all cling to it with an instinctive pertinacity, and make its continuance and preservation an object of the extremest solicitude. For ages was man, the highest of the animal creation, busied in straining his faculty of reason in the pursuit of an idle chimera —the dream of perpetual youth; nor was the visionary task altogether abandoned even when information, to a very great extent, had become more generally diffused, eradicating for ever many other opinions equally as fallacious and ridiculous. That panting for an earthly immortality, so congenial to his nature, was destined to survive longer, and to form a melancholy monument of his weakness, too glaring to be either controverted or denied, showing that minds of the highest order were still to be found joining in the chase of an illusory phantom, existing only in the visions of their own fervid imagination,—a shadow without real form or substance. But, fortunately, at length, as in the former instance, where the prevailing mania for the transmutation of metals faded before a better knowledge and a love for chemical science, so did this universal rage for perpetual adolescence succumb to a more extended insight into the great leading physiological principles which regulate and govern the several component parts of the human economy. Inquiries instituted at first merely for the purpose of obtaining a more comprehensive idea of the structures of the living body, in order to make themselves masters of some surer grounds to enable them to reduce their favourite theories to actual practice, gradually accomplished a mighty revolution in sentiments that had hitherto reigned without contradiction, for it taught them accurately the true nature of the material they had to deal with, and by slow degrees they were led to understand that the law of creation was omnipotent in all things, enforcing with unrelenting hand decay as the penalty of existence,—an inevitable consequence, against which man in his pride might struggle, but to sink at last beneath its stern decree, powerless, defeated, and abashed.

When this monstrous error was finally dissipated, and a complete revolution in sentiment thoroughly established, mankind would appear to have assumed views more circumscribed, and more in unison with their own limited faculties. The

philosopher at once sedulously devoted himself to the task of investigating the causes productive of that effect, which their predecessors had so long, but so unsuccessfully, laboured to avert; and hence originated a minute and critical examination into the tissues and general organism of the human body: their uses, peculiar functions, actions, &c., with the probable reasons that might be rationally assigned for their gradual impairment; and ultimately their complete incapability for performing those offices for which they were originally so admirably adapted. These attempts at physiological inquiry, though rude and conjectural at first,—the natural condition of every new scientific subject in its infancy,—by degrees assumed a certain shape and more decided tone, and as they, in the commencement, dealt only in what was palpably demonstrative, they experienced but little difficulty in impressing their views, and in winning over numerous adherents to enter upon and pursue the same path with themselves. The success which attended those early efforts was really astonishing; and the unprejudiced physiologist of the present day would be deficient in that candour which ought to be his chief characteristic, were he to attempt to deny the vast benefit which he has repeatedly derived from their labours, forming, as they do, in many instances, the solid groundwork on which are based the majority of those minute and elaborate inquisitions, executed with the aid of modern inventions, and perfected by a more intimate acquaintance with those topics generally.

Amongst, however, the numerous and varied objects that naturally presented themselves in the course of this novel inquiry, there was one so conspicuous that it could scarcely fail to awake and arrest the attention of almost every investigator, and this was the very different condition and appearance of the blood as it existed in the vessels of the human body; and the marked alteration that occurred in it on being removed from that vital laboratory. This change in its character was a phenomenon too obvious and remarkable to escape notice for a moment; and as all experiment showed that it was one not of casual occurrence, but uniform in its nature, it of course became a matter of universal interest to endeavour to afford some rational explanation of the causes that were capable of producing it. Hence arose a multitude of opinions, each borrowing support from and depending on isolated facts, which appear at the moment to have struck the mind of the beholder as a means amply sufficient to account for the extraordinary transition occurring in the sanguineous system under certain circumstances; and as each was anxious to strengthen his own

peculiar views alone, by inferences drawn from a single phenomenon out of the many that are well known to exist,—it cannot be regarded as at all surprising if theories as numerous as they are in many instances diametrically opposed to each other, should be successively broached, to be as rapidly overthrown by others resting on a foundation equally as unstable, and quite as unfitted to bear the test of close and critical scrutiny. In the work before us, on the Cause of the Coagulation of the Blood, being the Astley Cooper Prize Essay for 1856, a special debt of gratitude is certainly due to Dr. Richardson for his masterly historical sketch of this subject; and in the first chapter, which he has wholly devoted to it, he has been most careful in enumerating the names of almost every one who has ever alluded to the matter, with the opinions advocated by each, summing up the whole with the following statement, which he considers to be a fair deduction, capable of being drawn from the arguments as advanced by each and all:—

"1. That the blood may, in very rare instances, coagulate in the ordinary way, *i. e.*, as a red clot, in the body during life.

"2. That the fibrin alone may separate in the form of concretions during life; this result being theoretically attributable (*a*) to mere slowness of motion, as in the aneurismal tumour: and as in some cases of slow death; (*b*) to absolute or relative increase of fibrin itself; (*c*) to a peculiar kind of coagulation arising from chemical or physical changes incident to the mixture of some poisons as pus with the blood; (*d*) to the neutralisation of those alkaline constituents of the blood, which hold the fibrin in chemical solution, by some free acid, as lactic, generated in the body superabundantly; (*e*) to an extreme tenuity of the serum in which the fibrin is distributed, with a feeble resistance against the natural attraction of the molecules of the fibrin and their consequent aggregation; (*f*) to a supposed disorganisation or disintegration of the blood, under the influence of certain diseases, as typhus and scurvy, whereby it has a tendency to divide into its constituent parts.

"3. That these depositions of fibrin may, like the buffy coat, differ in constitution; that they may be more or less coloured, from enclosing in structure modified red corpuscles; that they may become softened in structure, such softening being most marked in the centre; that they are often accurately moulded to the cavity which contains them; that they may be developed both on the venous and arterial sides of the circulation: that they sometimes become firmly adherent to the walls of the heart, by interlacing with the muscular fibres; and that, in some rare instances, the adhesion which they had formed with the heart may become united by vessels, so that the deposit itself ultimately assumes an organized character.

"*En resumé*, we see that in regard to the causes of the coagu-

lation of blood, both in and out of the body, the explanations that have been adduced rest on one or other of three grand theories: the vital, the physical, the chemical; and it is to be observed, that in the order of time in which these theories have influenced physiological inquiries in general, so have they specially influenced those which relate to coagulation. How far each or all of the speculations which have now been recorded have a bearing on the question at issue, is a subject which must be considered in a distinct chapter."

In a work of the description of the one before us, consisting essentially of a series of consecutive experiments instituted for the purpose of establishing a variety of hypotheses, it would be clearly impossible to attempt to analyze each article individually, as the mere want of space would form an insuperable barrier to such a course of proceeding, and all that we can therefore undertake to do is to make such a selection from the whole as will be sufficient to afford the reader a comparatively fair idea of its general character, and the spirit in which it has been composed. We accordingly take the following as intended to illustrate the effects of copious water-drinking on the blood:—

"Experiment xl. *Effects of copious Water-drinking.*—Bearing on the subject of the effect of dilution of blood with water, I had the opportunity of making some interesting observations on the human subject. On November 29th, 1856, a boy, aged 16, came before me with diabetes. He passed about seven quarts of urine in the twenty-four hours, but much the greater part in the night-half of the time. The amount of fluids which he took averaged also about seven quarts, but he was always thirsty. The urine yielded abundant evidence of sugar. I made it a point in this case to get the patient to do without drinking water as long as it was possible. He could manage to abstain three hours. In this state I drew from his hand a few drops of capillary blood, and watched the process of coagulation. The blood was dense, rather dark, and coagulated in thirty seconds. The blood from the puncture ceased to flow in nearly the same time. When this observation was concluded, I allowed him to drink water freely. He took off two quarts greedily, in half-pint draughts, at intervals of two minutes. Five minutes after the last draught I made a new incision, and drew off in the same way as before a few drops of blood. There could be no doubt as to the physical differences between this and the blood previously drawn. It was more fluid, it remained fluid for five minutes, and ultimately rather dried than coagulated. The wound last made continued to give out blood; and the wound previously made recommenced to bleed. Both wounds continued bleeding for several minutes, the fluid retaining its liquid character; at last I stopped the flow by pressure. I examined the red corpuscles in

each specimen of blood. The difference here again was well-marked. There was not much variation in size, but important variations in shape; those of the latter or watery blood being irregular, some oval, others many-sided, and a few stellate. These also continued in motion for a much longer time.

"This experiment, as illustrating the rapid absorption of water by the exhausted body, and the effects of the water on the blood, throws much light on the cause of death in instances where fatal effects have occurred in exhausted persons, after the drinking of fluids too copiously. I may remark that this patient always showed symptoms of faintness on replenishing himself overmuch with water upon a prolonged abstinence. He exhaled ammonia freely by the breath at all times."

To show the results of different degrees of temperature on the blood, we find a little farther on the following ingenious contrivance described, which would appear to have effected the proposed object most effectually:—

"EXPERIMENT LII. *Effects of Temperature.*—I took three thermometers, which kept tune together, when tested at different temperatures. I also fitted up three glass cups, so that each cup should inclose a second cup for holding blood. I then surrounded one of the inner cups with water at 56° Fahr.: a second cup I placed in water at 140° Fahr. The temperature of the external air was 72° Fahr.

"After taking the exact temperatures of the hot and cold waters, as noted above, I removed the thermometers from the fluids in which they had been immersed, and placed them upright in the two empty cups intended to receive the blood. The mercury in each thermometer at once changed its position. That in the cup surrounded by cold water rose to 65°; that in the cup surrounded by the heated water fell to 126°. At this moment an assistant received blood from the neck of a sheep, and instantly filled the two cups; and also a third cup, of the same size and shape, which was simply exposed to the temperature of the air, and in which the thermometer stood at 72° Fahr. As soon as the blood enveloped the thermometers, the mercury changed position in all of them; the two thermometers, which had stood respectively at 65° and 72°, rose to 92°; the thermometer, which in the cup surrounded by hot water had stood at 126°, fell to 95°, and did not reascend until after coagulation had occurred. The blood in each cup coagulated in fifty seconds. I was unable to notice any difference at all in the period of coagulation; but the clot, at the end of one minute, was decidedly firmest in the blood which had been placed at the highest temperature."

As a simple substitute for the slow and tedious operation of the air-pump, the mode explained below, would, we are con-

vinced, answer the purpose exceedingly well, and in expert hands could scarcely fail of success:—

"I had made and fixed on a stand a strong glass vessel, like a cupping-glass inverted, and having a firm and narrow neck. For the neck a movable stopper was made, from which a small tube depended for holding a bit of cotton wick.

"When the instrument was being used, the wick, charged with spirit for ignition, was held ready at hand by an assistant. The portion of blood required was then received from the animal, and at once poured into the glass, immediately upon which the assistant inserted the stopper with the wick ignited, but slowly, so that the enclosed air could escape. When the stopper required to be taken out, a turn of the little stop-cock at the top let in air, and allowed its easy removal.

"Although by this exhausting cup less air is removed than by a good air-pump, the simplicity and directness of the experiment are so well marked, that the disadvantage is more than counterbalanced."

The following is a conclusive experiment with respect to the condition assumed by the blood on being occluded in veins, and the inferences drawn from it are further corroborated by several others, to which we must beg to refer the reader:—

"Experiment cxlv. *Occlusion of Blood in the Veins.*—An animal (a dog) was dosed at night with a drachm of solid opium. He became partly comatose in a few hours, and in the morning was found conscious, but unable to move, cold, and very little alive to pain. A little blood drawn from the ear coagulated in six minutes. He had had given to him full doses of opium for several previous days. After the blood was drawn from the ear, he was slowly killed, by being made to breathe a diluted dose of prussic acid vapour. The object in producing gradual death by narcotism was to render the blood slowly coagulable, so that its period of coagulation in different parts might be observed systematically and deliberately in an animal newly dead. The instant the animal had ceased to breathe, the external jugular vein was laid bare, tied in two sections, and recovered with the skin and soft parts. Next, the abdomen was laid quickly open, the intestines were turned aside, and the vena cava inferior was tied in two places, so as to leave an inch of space, between the ligatures, full of blood. The intestines were immediately afterwards replaced in exact position, and the wound in the abdominal walls was stitched up. The animal was now let alone for one hour; at the end of this time the jugular vein was examined, and the lower segment was punctured; the blood in it flowed out in the fluid state, and coagulated in seven minutes after removal. At the end of fifteen minutes the blood in the upper segment was drawn: it, too, was fluid, but coagulated in seven minutes on exposure. The chest was then opened. The heart on the right

side was distended with blood, and, when the cavity was laid open, this blood was found principally fluid; but in one or two points (in the auricula, for instance, and in the pulmonary artery) there were small loose coagula. Received in an evaporating dish and exposed to the air, this blood all coagulated in one firm clot in ten minutes. The blood in the left cavities of the heart was small in amount, and had fully coagulated.

"After a lapse of time, amounting to two hours and a quarter from the death of the animal, the inferior vena was now again exposed. By this time the blood in all the exposed and opened veins was coagulated; but on puncturing the segment of the cava which had been included in the ligatures, the blood it contained flowed out in a free fluid stream, and coagulated in ten minutes on exposure to the air."

To meet a certain class of objections as detailed by the author, it became necessary for him to show the effects of circular motion on the blood, and hence he was obliged to devise some mode by which he might imitate the current of the circulation in the living body. This, after a few trials, he at length successfully accomplished, after the manner detailed below:—

"*Blood in Circular Motion.* Those who have supported the theory that blood is kept fluid during life by mere motion, have contended that the agitation of blood in a closed vessel is in no way a fair experiment; since the column of blood merely shaken in a bottle is in some points at rest, and is not moving on in the same equable and even circuit as in the body during life. This objection was plausible, and required to be met by direct experiment. It occurred to me, therefore, to devise an artificial circulation, and, by mechanical means, to keep a column of blood in steady circular play. I should waste time by describing the many mechanical difficulties which I met with in this simple experiment. The difficulties arose mainly from the impossibility of making valves that should exactly imitate those in the body, and should play as well. In one apparatus, which was worked by a double piston, I used metallic valves, and succeeded in sustaining a water current through it. With blood, however, it would not answer at all; for the fibrin adhered to the valves at once, and blocked up the tubes. At last I thought of the single heart of fishes, and took this in some measure for a pattern, with the difference of inventing a mode by which valves within the tubes might be dispensed with. In the end, an apparatus was made up of two bags of thin vulcanized India rubber. The bags were each capable of holding an ounce and a half of fluid. From each bag ran out two tubes of the same material. These tubes were three inches in length, and were slipped on to two curved glass tubes, so as to complete a circuit with the bags, which were opposite to each other in the semi-circumference

on each side. The circuit thus made was fixed on a strong wooden stand. A light beam extended across the table from one bag to the other; each end of the beam being provided with a kind of cushion adapted to the bag. The beam turned vertically on a pivot in the centre, and was provided with a handle at each end for assisting the movements.

"By simple spring valves placed at opposite positions at the termination of each of the arms of the beam, the passage of the fluid was kept in one direction; and, by the movement of the beam first on to one bag, and then on to the other, the onward motion was secured.

"When the apparatus was to be used, blood freshly drawn was poured into the circuit. In my first instrument, which was sent in to Guy's Hospital, and which was hastily made, the blood was introduced by slipping off one of the India rubber tubes from the glass tube, and then rejoining them. In this way Experiment CLXIX. was performed. Afterwards, I improved the apparatus by fitting to it at the upper part a funnel with a stop-cock, which stood upwards at a right angle with the tube. When the apparatus was filled, the stop-cock was turned, and the motion of the beam was commenced. By a little care and dexterity, the process of filling was performed in half a minute.

"When freshly drawn blood was introduced so as to fill the apparatus in the way described above, and was set in motion in the circuit by the action of the levers, it never coagulated so as to stop the current; but this ultimate result was due chiefly to the gradual separation of the fibrin, which deposited on the inner surfaces of the India rubber bags. At the same time, steady circular motion of this kind, in a closed circuit having the least possible tendency at any point to retard the current, showed a marked effect in delaying coagulation: for I found that the separation of fibrin in the apparatus was not immediate, since blood, drawn in a fluid state from the apparatus after it had been kept in motion ten minutes, and in one instance a quarter of an hour, coagulated on removal. But it has an interesting bearing on some points in pathology to know that, after a longer lapse of time, the fibrin commenced to be deposited in the interior of the bags, whence it could be washed in fine filamentous threads. After this separation, the blood, of course, remained permanently fluid. The blood-corpuscles were uninjured."

Passing on to the appendix, we select an experiment or two showing the effect produced by lactic acid on animal bodies; and at the same time we must be permitted to express our regret that the whole article is by far too long to be fully extracted:—

"The instrument is a simple hollow needle, attached by its blunt end to a tube connected with an India rubber injecting bag.

During the operation, the animal is held down on his side; the needle, charged to its point with the acid solution, is cautiously introduced through the abdominal wall in the flank; and the fluid, previously raised to a temperature of 96°, is slowly thrown in. Prior to the operation, the animal is kept for a few hours from food and drink.

"I proceed now to the details of experiment.

"EXPERIMENT CCCC. *Injection of Lactic Acid.*—A full-grown healthy cat was placed under the influence of chloroform. The external jugular vein was laid bare, a small tube was introduced into it, and an attempt was made to inject four drachms of the acid with the same quantity of water. The operation, for reasons I have explained above, as well as in Experiment CCLX., p. 252, did not succeed. I therefore at once closed the wound in the neck, put a ligature on the vein, and, making a lancet opening into the peritoneum, threw in seven drachms of the acid, with eight of water. The operation was easily done, and no fluid was lost. The animal soon regained consciousness, and recovered from the effects of the chloroform. During the first hour after the operation she was breathing at the rate of forty-five per minute; the surface of the body was cold, and she was restless. Two hours later the respirations were forty-two per minute, and the heart-beat so irregular and irritable that it could not be counted accurately. About six hours after the operation, she was left in the same condition for the night, being provided with a straw bed and food. In the morning she was found dead, and with the body in a state of intense rigidity.

"*Post-mortem Examination.*—The peritoneal surface showed no sign of inflammation, and contained no trace of the injected fluid. The liver on its exterior was rather pale, but internally was congested. The kidneys and spleen were congested. The lungs were entirely free from congestion, but of a colour too much approaching to red to be natural. The blood in the veins and right side of the heart was of a dark, slimy, soft, and cheesy character, much as I have described as occurring when lactic acid is added simply to blood. The right side of the heart was healthy. The left auricle contained a small coloured coagulum, which interlaced feebly with the muscular structure of the wall. The left ventricle contained some fluid blood. The mitral valve was greatly thickened, red, and (I am indebted for the term, as I shall afterwards explain, to my friend and colleague Dr. Halford) œdematous. On the surface of the valve there was a small fibrinous deposit, which adhered to its position by means of an under layer of a white glutinous fluid, the like of which I found afterwards could be made to exude from the valve on puncturing it with a needle. The surface of the endocardium lining the left ventricle had an intensely vascular appearance, resembling, in fact, bright red velvet. The pericardium was dry and slightly injected. There was no trace of mischief in the joints, and the brain was normal.

EXPERIMENT CCCCI. *Injection of Lactic Acid.*—I injected into

the peritoneal cavity of a full-sized terrier dog, through a lancet opening, one ounce of lactic acid with two ounces of water, at eight P.M. The animal was not narcotised, and seemed to suffer but little from the operation. The fluid was all retained. Prior to the operation, the chest was examined with the stethoscope, and the respiratory and heart sounds were found quite normal. On the following morning he was languid, and seemed to care little for food: there was no pain on pressure of the abdomen, nor fulness. The heart-beat was quick and sharp; the second sound seemingly accented; the first obscure. The respiration was natural. At eleven A.M., I injected another half-ounce of the acid with an ounce and a half of water; the operation produced no expression of pain. At three P.M., Dr. Snow, Dr. Beck, Dr. Graily Hewitt, Dr. F. Webb, Mr. Spencer Wells, and Dr. Rogers, saw the animal. We distinctly made out that the second sound of the heart was prolonged, and the first obscure; but we could not decide as to the existence of a *bruit*. The sclerotic vessels were greatly congested. The animal lived, with much the same symptoms, till early the following morning, when he was found dead, but lax and quite warm.

"*Post-mortem Examination.*—The cadaveric rigidity was less marked than in the cat. No injury to the intestines had resulted from the operation. The peritoneal lining showed no trace of injection or inflammation, but was rather paler than usual. The mucous surface of the intestines was in parts vascular. The liver was rather shrunken and pale, dense in structure, not inflamed. The spleen was normal. The kidneys were rather congested, and their capsules were intensely injected, the vessels being beautifully delineated. The bladder was contracted and empty, and its lining membrane was pale. The bronchial membrane was free from congestion; but the lungs were vascular, dark, and condensed, except the surface of the upper lobe of the left lung, which was of a bright red, contrasting strongly with the other parts. The pericardium contained no trace of fluid, but was unusually dry, and, like the capsule of the kidney, had its vessels finely injected. Held up to the light, the membrane had a bright vermilion tint. The heart was distended on both sides, but most on the left side, with clotted blood. The endocardial surface on the right side was natural; but one segment of the tricuspid valve was swollen several times beyond its usual size from œdema, and was of a bright red colour. The thickening was irregular. The pulmonary semilunar valves were natural. On the left side, the endocardial surface lining the ventricle was throughout of a brilliant red colour, having a velvety appearance, and contrasting strongly with the lining membrane of the right ventricle. The segments of the mitral valve were thickened, œdematous, and also of a bright red colour; while, around their free margins, were several beads, varying from the size of a pin's head to that of a millet-seed. The aortic valves were of a deep red colour; their free borders were thickened and everted. There were no fibrinous coagula. The muscular structure of the

heart generally was more than usually red. The external vessels of the brain were congested; but the brain structure was natural. Several joints were laid open, but presented no abnormal appearance."

We would conclude our notice of this book by most sincerely expressing our conviction of its real and intrinsic excellence. Each single page of it abounds in matter of profound importance, not only to the mere physiologist, but likewise to every one who is anxious to be considered thoroughly educated and accomplished in the details of his profession. Here will be found no idle theories, borrowing their support from ingenious sophistry and subtle argument, but a vast accumulation of facts resting upon solid data and absolute experiment, the accuracy of which can at any moment be tested by every one who can boast the same amount of energy, talent, and mechanical ingenuity as has been evinced by the author in the course of his long and arduous inquiries. Still, as far as we are ourselves concerned, we unhesitatingly receive every one of the conclusions at which Dr. Richardson has arrived, with the most implicit faith and confiding reliance, for in every line of his book there breathes such a tone of candour and truth as to leave no room for even a shadow of doubt on any of the subjects which he has so admirably handled.

The Enlarged Prostate, its Pathology and Treatment; with Observations on the Relation of this Complaint to Stone in the Bladder. By HENRY THOMPSON, F.R.C.S., M.B. Lond., &c. &c. London: Churchill. 1858. 8vo, pp. 320.

WHEN we recollect the fecundity with which the press has of late years yielded books on diseases of the urinary organs, and that its offsprings in this line have borne in general so much of the same character—being mostly treatises on stricture of the urethra—it is refreshing to find that when a new birth has taken place, though it belong to the same family, it should exhibit an altered stamp. Prostatic disease is certainly not amongst the least important of the obstructive affections of the male urinary apparatus—the pain, trouble, and annoyance attending it—the difficulty experienced in its management, and its being in one form an almost universal visitant—are sufficient to claim for it a large share of attention; yet writers have not, in a comparative sense, done much in this particular field. We are therefore happy to find that Mr. Thompson, whose excellent work on the Pathology and Treatment of Stricture of the

Urethra we have already had the pleasure of noticing in this Journal, has directed his energies to the elucidation of prostatic disease; and we shall merely observe, *in limine*, that if he have brought to bear on the present subject an amount of labour, learning, and ability equal to what he displayed in the construction of the volume alluded to, he has furnished the profession with a work of no inconsiderable value.

The first chapter is devoted to a description of the anatomy of the prostate gland, which is dealt with in the manner best calculated to elucidate the pathology of the organ. The points worthy of most notice in this chapter are the observations on the "third lobe," and on the measurements of the prostate gland, especially as they relate to lithotomy. In reference to the first point, the author says:—

"I am aware that by most authorities in this country the existence of 'the third lobe' is regarded as a fact in normal anatomy. A very extended investigation which I have prosecuted, with a view to the determination of this and other doubtful points in the anatomy of this organ, has convinced me that there is no part of the healthy prostate entitled to enjoy this distinctive appellation."

In a note appended to this statement here made, the author mentions that he has drawn his conclusion from the result of upwards of sixty dissections made by himself. He further observes:—

"Any appearance of a lobe in this situation must be regarded as belonging not to normal, but to morbid anatomy, a slight development of it being usually attended with some signs of obstruction to the function of micturition."

And concludes his remarks thus:—

"In an unnatural condition this portion is found enlarged, and then presents an appearance which is not unlike that of an independent lobe, but as this is a deviation from the normal state, the use of the term to the healthy part appears to be indefensible, and I shall in future speak of it by the term already proposed, viz. that of 'the posterior median portion.'"

The whole chapter is ably worked out, and is, in our opinion, well worthy the study not merely of the anatomist and pathologist, but of the practical surgeon, who should possess a clear and accurate knowledge of the normal condition of an organ which, when diseased, he undertakes to treat.

The second chapter treats of the anatomical characters of the senile enlargement of the prostate gland:—

"Enlargement of the prostate may take place as the result of several different pathological actions or processes; and, therefore, presents varieties, possessing very different characters, both as to the course observed in the progress of the disease, and the structures which may be revealed by anatomical examination of the organ after death.

"These varieties may be conveniently separated into two different classes.

"I. Enlargements, which are constituted by an excessive formation or unnatural arrangement of elements analogous to those of which the prostate is composed.

"II. Enlargements, occurring from the deposition of heterologous elements."

This is an excellent division of the different varieties of pathological changes which occur in the prostate. Under the first division is, of course, comprehended the hypertrophy or ordinary enlargement of the gland. With respect to the "external form" exhibited most usually by the latter, the author furnishes the following classification, which is founded on 123 specimens, carefully examined :—

"Common forms of the affection.

"I. General enlargement of prostate, that is, both lateral lobes and the posterior median portion pretty equally enlarged, is present in 74 preparations of the 123.

"II. General enlargement of prostate, but the posterior median portion enlarged in greater proportion; in 19 preparations.

"III. General enlargement, but the right lobe predominating, and very decidedly larger than the left; in 8 preparations.

"IV. General enlargement, but the left lobe predominating, and decidedly larger than the right; in 11 preparations.

"Uncommon forms of the affection.

"V. The lateral lobes only enlarged; in 5 preparations.

"VI. The anterior portion only, or chiefly enlarged; in 3 preparations.

"VII. The lateral lobes and anterior portion enlarged, not the posterior median portion; in 3 preparations."

So far as the structural changes developed in the prostate of advanced life are concerned, Mr. Thompson adopts the following classification :—

"I. Enlargement depending on general hypertrophy; that is, affecting the whole, or, at least, a considerable part of the prostate.

"1. Affecting the entire structure of the organ.

"2. Affecting the muscular tissues only.

"II. Limited hypertrophies in the form of tumors and outgrowths.

"1. Circumscribed tumors of the prostate.

"2. Outgrowths."

The latter pages of this instructive chapter are occupied with arguments and observations calculated to establish the analogy existing between tumours of the prostate gland and of the uterus. The analogy is made to rest primarily upon the now recognised physiological fact, that the uterus and prostate gland are "the morphological equivalents" in the two sexes; but other arguments, pathological and otherwise, are adduced in support of the same views.

In the third chapter we have the causes of senile enlargement of the prostate fully considered. After alluding to the opinions held by various authors, and showing that those circumstances, such as stricture, gout, syphilis, sexual excess, &c., &c., which have generally been viewed as the causes of hypertrophy of the organ, hold no relation to it, he labours to prove that this enlargement of the prostate is identical in its nature and causation with that of the uterus; and certainly if the arguments he employs be not *weighty*, they are ingenious and interesting. The author begins his remarks thus:—

"There is but one other organ in the body which is similarly constituted, as regards the nature of the constituent tissue, and in the manner of its aggregation, a fact enlarged upon in the preceding chapter. The uterus, like the prostate, is composed of the inorganic muscular tissue distributed in thick strata, so as in either case to form a thick mass, not in thin planes, as found in all the other organs in which this tissue appears. The tendency to become the seat of local and general hypertrophy, of isolated tumors and outgrowths of a special character, which both organs equally manifest, has also been demonstrated. Starting from this remarkable fact, it is difficult to resist the inference that this tendency to overgrowth, this disposition to generate fresh elements identical in character with those proper to the structure of the organs, has a source common to both, and perhaps inherent as a kind of structural, or perhaps functional necessity."

Next to the treatment of any disease, the symptoms which evidence its existence must be viewed as the most important consideration connected with it; still, how careless are practitioners often in looking for them, in how slovenly a manner are they noticed, and how few train their perceptive powers so as to allow of a ready recognition of them when obscure. The tendency at present is to treat symptomatology rather with contempt; but how can a correct diagnosis be arrived at

without previous recognition, and analysis of symptoms? and though the treatment directed against individual symptoms is to be condemned as unscientific, and often pernicious, still, how often is the *best* diagnostician, owing to the obscurity of the *disease's nature*, compelled to look on and do nothing, if he disregard the treatment of mere symptoms! Again, how frequently might disease be checked or ameliorated, or its progress delayed, by a recognition of symptoms, while as yet they are imperfectly pronounced. These observations apply especially to prostatic enlargement.

"In the earliest stage of chronic enlargement of the prostate there are no symptoms sufficiently marked to attract the attention of the patient. It is probable, indeed, that a very considerable period, varying in different cases from a few months to some years, is passed between the actual commencement of enlargement and the occurrence of anything which is observed to be unusual in the act of micturition, or of any kind of derangement arising from the organic changes which are taking place. The length of time which elapses between the onset of disease and the manifestation of symptoms depends mainly upon two conditions: first, upon the character of the enlargement itself; and, secondly, upon the constitution of the patient."

Now there is no doubt but that in every case of incipient chronic enlargement of the prostate, some one symptom, at least, *does* exist; for it is impossible that this organ can undergo, not to say enlargement, but organic alteration of any kind, without giving some manifestation of its existence; it is not the absence of manifestation, but our inability to recognise such; and this is due in great measure to, as has been already observed, the superficial and slovenly manner in which we investigate the symptoms of disease. If any substantial good is to be done in the treatment of prostatic enlargement, the abnormal change must be recognised *early;* if we wait for the advanced stage—when partial retention of urine and chronic cystitis are present—we can do but little to relieve the symptoms, and nothing to retard the progress of the disease. The first symptom, almost invariably, is increased frequency of micturition. Difficulty of micturition may also exist, but this depends altogether on the direction the enlargement takes; in no case, however, can the former symptom be absent, since the slightest deviation from the normal condition of the prostate gland cannot fail to be a source of irritation to the bladder.

In describing the advanced stages of prostatic disease, the author properly draws attention to a mistake frequently fallen into by ignorant practitioners, of ascribing the involuntary

dribbling of urine to *incontinence*, instead of considering it as an overflow—the result of a distended bladder—the condition, as he has observed, "so far from being one in which the bladder cannot *retain*, is one in which it *retains* too much." But he has omitted to allude to one source of involuntary escape of urine, sometimes met with in cases of prostatic enlargement, and which is of practical importance, since it does not imply over-distention of the bladder, viz., retention of urine in the prostatic urethra, from the latter having undergone dilatation. This condition readily proves a double source of deception,—first, by affording the indication of incontinence; second, by leading to the belief that an instrument has entered the bladder, owing to the escape of urine, when it has not passed the obstruction at the neck of the organ.

The fifth chapter treats of "the effects of enlarged prostate in relation to the function of micturition, retention, incontinence, enlargement, and overflow." The author's observations on these several points are worthy of attention.

We shall now proceed to the seventh chapter, in which a very elaborate consideration of the treatment of senile enlargement of the prostate is begun. It has been asked, what advantage can arise from devoting much consideration to the treatment of a disease which is *incurable*,—one, too, in which the alteration of structure is the result of certain senile laws. As well might we endeavour, it has been said, to check or alter the many other evidences of advancing age, as to turn senile prostatic disease from its onward course. Hence it is a common custom to "do nothing" in this disease as soon as the symptoms are recognised as those indicative of senile enlargement of the organ; the patient is told to submit placidly to all the annoyance arising from it, and that it is useless to resort to any treatment until, if such should occur, a necessity arise for the employment of the catheter. But it must be remembered that the treatment is directed *mainly* against the *effects* of prostatic enlargement, and these we can, by timely and proper management, not only prevent, but remove. Early and judicious treatment of this disease on the part of the surgeon, and the observance of caution and prudence in the general mode of living on the part of the individual, may almost indefinitely delay the progress of the disease, and retard the *effects* of its advanced stages. But more than this, some, though comparatively *much less*, good can be accomplished by treatment directed against the disease itself. Mr. Thompson makes the following useful division of the treatment:—

"1. Treatment for the purpose of obviating the results of obstruction caused by enlarged prostate.

"2. The general, or constitutional, treatment and management of patients with enlarged prostate.

"3. Treatment directed against the enlargement itself."

He truly observes, that when it becomes obvious the bladder is unable to *completely* evacuate its contents, "the first great principle to be followed out is this,—that it is necessary to insure the complete removal of the urine from the viscus at least once a day." If this rule be not observed, the quantity of residual urine increases until partial retention sets in, cystitis is induced, chemical alteration of the urine occurs, and atony of the muscular fibres of the organ. With reference to the practice of retaining a catheter in the bladder, the author observes—

"It has been considered by some that an advantage is obtained by permitting a catheter to remain in the bladder for days together. This appears to me erroneous. There are two hypotheses on which the advice is grounded. The first is, that by permitting the bladder to remain empty, or nearly so, we encourage it to regain its contractility, assumed to be lost or impaired by overstretching. This, however, is not the true pathological condition which causes retention in these cases. There may be some little loss in the muscular power of the vesical coats, but it rarely amounts to much. The material obstruction at the neck caused by the enlarged prostate, and not any 'local paralysis,' as it is commonly termed, is the sole, or almost sole, occasion of the urinary difficulty. This has before been explained and illustrated. Hence, from this hypothesis, the practice referred to gains no support. The second is, that the constant pressure of the catheter promotes absorption of the substance of the tumour, and so tends to the material improvement of the patient's condition. But the fact is, that the tendency is much more to ulceration than absorption, after the manner intended. Granting, for argument's sake, however, that the desired action can be thus insured, it has still to be proved that the object is gained, at an expense of pain and confinement, to say nothing of risk, sufficiently small to render the result an undoubted and valuable acquisition. Experience negatives the supposition."

Our experience does not exactly coincide with Mr. Thompson on this matter. We have seen many cases in which control over the bladder was gained *only* by adopting the plan of keeping an instrument in the viscus for some weeks; and we have recently witnessed three, which tend strongly to illustrate this fact. In two a catheter was worn for six weeks, and

in both the bladder regained its expulsive power; in the third, the patient refused to submit to the inconvenience arising from this plan, and though the instrument was regularly introduced at stated times, no substantial good in the one particular was procured. We cannot pretend to say why the retaining of an instrument in the bladder should act so favourably. Mr. Thompson may be correct in stating that atony is not the cause of the retention, but we think him in error in believing that "the material obstruction at the neck caused by the enlarged prostate" is the "sole, or almost sole, occasion of the urinary difficulty." In many of the cases of retention from diseased prostate we have met with, the *most complete* have been those in which there was not the least evidence of obstruction at the neck of the bladder, and where the organ lost its contractile power *abruptly*, there having been no previous difficulty in expelling the urine.

"A very frequent accompaniment of enlarged prostate is the appearance of blood in the urine. It may be simple, unprovoked hemorrhage from some part of its surface, or it may be the result of passing an instrument. In the latter case, it will sometimes take place although passed with the greatest care and without pain, so ready to bleed is the organ in some states when enlarged; but this is an exceptional occurrence. It may be caused by the patient moving about after the catheter has been passed, while it still remains in the canal; supposing, for example, that it has been passed in the recumbent position, and that the patient rises to evacuate the fluid. Some cases there are in which, although the instrument passes without obstruction, or even causing any painful sensation, it is, nevertheless, exceptional not to see a little blood at the time, or soon afterwards. *A fortiori*, hemorrhage may occur, and to a large extent when the employment of the catheter is difficult, or occasions pain. This circumstance is said to indicate ulceration of the organ. It may be so in rare cases, but I know that it often happens when no breach of surface can be found after death. It is not improbable that the hemorrhage takes place from enlarged and congested vessels in the mucous membrane of the bladder, the veins from which are often pressed upon and probably obstructed when the prostate is enlarged."

Hemorrhage is undoubtedly one of the most unpleasant and serious consequences of senile prostatic enlargement. Apart from that which results from catheterism, it acknowledges many causes: it may be the consequence of a congested state of the lining membrane of the bladder, or of the presence of a calculus in that organ; or it may come from the prostate itself. In the latter instance the bleeding is sometimes profuse, and

where it regurgitates into the bladder, and coagulates there, the case becomes most troublesome and difficult to relieve. Coagulated blood in the bladder, where there is a large prostate, and much difficulty in introducing an instrument, furnishes one of the most dangerous and difficult cases the surgeon can encounter. We have met recently with a case of this nature. The patient is a man aged 80 years. For the last three years he has been liable to attacks of hematuria, the source of which is referrible to the prostate gland. Every attack seems to have been induced by cold; the bleeding was arterial, and always considerable in amount, and it was accompanied by smart pain at the neck of the bladder during micturition, and tenderness on pressure over the hypogastric region: the hemorrhage was *active*, and such as to lead to the idea of its being the result of rupture of a large-sized vessel. The last attack was the most severe; large quantities of blood in a coagulated state were passed after violent forcing, and there soon arose evidence that the bladder contained more blood than it could expel; at length, complete retention occurred, and the case became one of great urgency. The patient was so weak from loss of blood that he was unable to stand, in which position the introduction of the catheter was found on previous occasions to be effected with the greatest ease. After some trouble, however, the difficulty was surmounted, and a good-sized catheter was passed while the patient reclined, and by means of an exhausting-syringe the bladder was emptied. The instrument was retained, and every two or three hours it became necessary to inject warm water and apply the syringe, owing to the rapidity with which the blood collected. The only effectual remedy for checking hemorrhage in these cases is gallic acid, but it must be administered in large doses; less than ten-grain doses we have never found sufficient in cases of urgency. In the foregoing instance not the least impression was made on the bleeding until the dose was raised to ten grains, and by a single dose in that amount, bleeding, which on two or three occasions occurred since, was immediately arrested.

So far as the special treatment of the enlargement itself is concerned, no good, in a purely curative sense, can be gained thereby; but there is no doubt that much advantage arises from the adoption of a certain *medical* course. The occasional application of leeches to the perineum and anus, and the employment of iodine suppositories and enemata, will remove congestion, and diminish the bulk of the tumour. We have no confidence, however, in the efficacy of mechanical means. The

instrument for exercising compression, invented by Mr. Thompson (a description and plate of which are given at pages 158 and 159 of his book) is ingenious, and worthy of attention; but we are at a loss to understand how it can act in doing more than dilating the prostatic urethra, while we can readily conceive how much mischief might result from its employment in many cases. We know that in other morbid growths compression, to be effective, must be *firm*, *steady*, and *continuous;* there must also be a certain degree of *counter-resistance:* but these conditions cannot be gained in the case of enlarged prostate gland. The compression should certainly be continued for a considerable time, or be repeated frequently; still we find that in Mr. Thompson's method it is exercised for not more than five minutes every day. The instrument can be only viewed as a urethral dilator, and its mode of action as little different from that of a large catheter.

In the eighth chapter we have detailed the different methods of relieving retention of urine arising from enlarged prostate. Notwithstanding that many cases of retention are due to "congestion of the organ," we attach no value to any measures but the instrumental. Putting into requisition baths, opium, &c., is, in our opinion, not only useless but unwarrantable, on the ground of the loss of time it produces, and the consequent prolongation of the patient's sufferings. We fully concur in the sentiment expressed in the following quotation:—

"With the third and last indication comes the question of the catheter; not a question as to the propriety of using it forthwith, or of delaying; for of the former course there can be no question, but that of the kind of instrument to be employed, and of the best manner of overcoming the varied difficulties which may present."

In truth, the *kind* of instrument that should be employed is what the surgeon chiefly requires to know. The kind of instrument to be used must vary according to the shape of the prostatic urethra, as caused by the particular direction the enlargement of the gland takes; and as this varies exceedingly, it becomes necessary to possess catheters of various size and curve. Every surgeon has often found how easily he has succeeded in passing a catheter by slightly altering the curve, after failing repeatedly with one or more instruments. The particular description of catheter, whether metallic or gum-elastic, must, like the shape, be influenced by the direction the tumour gives to the canal. At page 169 of Mr. Thompson's book a plate is given in which are *seven* representations of pros-

tatic catheters, each differing *materially* in its shape, and having been recommended by a different surgeon; and this circumstance alone proves how varying is the shape of the prostatic canal, and the nature of the obstruction the instrument has to encounter. We have found, as a general rule, that the shaped catheter represented by Fig. *d* is the most suitable for the majority of cases of prostatic enlargement; but from the foregoing observations it is sufficiently clear that the surgeon must be provided with different-shaped instruments, and must patiently and perseveringly use them in succession, until he effects his purpose, since he cannot pretend to know in any given case the exact alteration the prostatic enlargement has made in the canal. But suppose "all attempts at gentle catheterism having failed," that the bladder calls for immediate evacuation, what course is to be pursued? In our opinion, perforation of the prostate is far preferable to the method of tapping the bladder; but it is not every case that will admit of this procedure; neither is it every surgeon who will be found to possess the necessary steadiness to carry it out effectually, and without risk. We have known extensive false passages between the bladder and rectum made in the attempt to effect perforation of the prostate; and we have frequently met with cases in which, though the catheter was held in the proper direction, the gland was merely *wounded*, not *perforated*, and thus serious hemorrhage to result. If the plan of perforating the gland be inadmissible, or that the attempt to accomplish it fail, the only alternative is, in our opinion, supra-pubic puncture of the bladder; to tap the bladder, *per rectum*, is impossible where the prostatic enlargement has gone to any considerable extent. We believe, too, that the operation alluded to is as free from risk as any of the other methods of puncturing the bladder: we cannot, therefore, agree with the author in the assertion, "that this is the most dangerous mode of reaching the bladder." Different from what it is in London, the practice in Dublin has been to tap the bladder above the pubis, even for retention from stricture, in preference to the operation through the rectum, and we have rarely found serious consequences to result from resorting to it.

We have now reviewed what may be considered the principal part of Mr. Thompson's work, that which treats of the senile enlargement of the prostate gland. The five subsequent chapters enter into the consideration of the inflammatory affections of the prostate; tubercular and malignant disease of it; and lastly, stone in the bladder, in connexion with enlargement of the gland.

Acute Prostatitis.—This affection is, in one sense, the most important of any to which the prostate is liable, because of its amenability to treatment. Being in general the result of gonorrhœal urethritis, which has transgressed the "specific distance," it sets in suddenly, runs its course rapidly, and, when energetically and promptly treated, ends in resolution. Where, however, the measures are inadequate to cut short the inflammation, or that they have not been adopted in time, or that a strumous diathesis exists, it glides into a chronic state which persists for a great length, and proves very troublesome to the individual, and difficult of cure to the surgeon.

"But acute inflammation may not be followed merely by a degree of chronic inflammation and induration, varying in extent, and tending to be permanent. Especially when neglected or inefficiently treated, or more than ordinarily obstinate, and when occurring in a naturally weak or delicate constitution, it may eventuate in suppuration of the organ, or it may subside, but so slowly and imperfectly, as it were, that a condition of subacute or chronic inflammation may be established, and remain during a long period."

Abscess of the prostate is, undoubtedly, a most important affection, and one that demands especial promptitude and decision on the part of the surgeon. Most certainly, if there be fulness and induration in the perineum, a free and deep incision into this part should be made without delay. When the abscess opens into the urethra, there is not only risk of infiltration, to a certain extent, but the case becomes very troublesome and tedious, the urine finds its way into the cavity of the abscess. This causes great irritation; the sac enlarges; and pain and difficulty in micturition, with puriform discharge, continue for a long time. We cannot exactly agree with the author that "spontaneous evacuation of matter by the rectum is perhaps as favourable, generally speaking, as through the urethra," since, for the reasons just assigned, the latter mode of evacuation is to be viewed as one attended with unpleasant consequences; while no evil can result from the rupture of the abscess into the rectum; when a fistula follows this latter event, the abscess has *also* opened into the urethra. We feel confident, however, that when the abscess bursts into the rectum, the matter, unless in exceptional cases, is formed outside the prostate gland; and indeed it is hardly possible to conceive that in the case alluded to by the author, where eight ounces of pus were discharged at once, so large a collection could have existed in the prostate gland, and especially not be followed by urinary fistula. We have seen two cases of prostatitis

in which, owing to the copious discharge of pus by the rectum, the absence of proportionate difficulty in expelling the urine, and the non-occurrence of urinary fistula, we felt convinced the matter must have formed in the areolar tissue between the prostate and rectum; a diagnosis as to the exact situation of the abscess would, for obvious reasons, be satisfactory in these cases; but we fully admit that "it is not, by any means, always easy to pronounce positively upon this point."

We are compelled to pass over the author's observations on malignant and tubercular disease of the prostate, and the "bar at the neck of the bladder," and shall merely stop to take a glance at the last chapter, before closing the volume.

Calculus in the bladder is, with the exception of renal disease, the worst complication with enlargement of the prostate; but happily the perfection to which lithotritic instruments, and certain auxiliaries to their employment, has been brought, renders the measure for its relief easy and certain, compared with what was the case some years ago. Though we cannot go so far as the author in believing that the coexistence of vesical calculus in enlarged prostate is so frequent, we are of opinion that this complication occurs oftener than is generally believed or known. The symptoms of both affections are in many respects identical; and where one disease is obvious, it is natural to view it as the "fons et origo" of all the manifestations, and even those which may be considered distinctive of calculus may, and generally are, more or less masked or modified by the prostatic disease. And even where the surgeon is sagacious enough to detect some of the symptoms of stone, how easily may he fail in feeling the foreign body satisfactorily with the sound. If the prostate be very large, or that there be a pouch behind the neck of the bladder,—if the calculus be small, or if it be encysted,—there is every chance of its evading the sound. We met with a case in which there was great enlargement of the prostate, considerable hemorrhage from time to time, and excruciating pain in passing urine, and even while it was being drawn off by the catheter towards the latter end. These circumstances were sufficient to suggest the idea that calculus might coexist; but though the patient was sounded *repeatedly*, and by more than one surgeon, no foreign body could be detected. On the post-mortem examination, a calculus of good size was found encysted in the upper fundus of the bladder; here, of course, neither lithotomy nor lithotrity could have been resorted to, but the case shows how, though calculus gives sufficient evidence of its existence by rational signs, it may be impossible to detect it by the only

conclusive means—the employment of the sound. Once the calculus has been discovered, the sooner the bladder becomes rid of so troublesome a tenant, the better; the longer the means to be adopted for its ejection are delayed, the more difficult will be their accomplishment, and the more serious the results to which they lead. Lithotrity, under the best circumstances, is more or less difficult of performance, and troublesome in its details where any enlargement of the prostate exists. It is perfectly true that—

"In lithotrity the enlarged prostate affords an obstacle more or less considerable to the easy introduction of the instrument; causes a little more difficulty sometimes in seizing the stone; and opposes a bar to the free discharge of the fragments."

Still, much can be effected, in spite of the difficulties afforded by the enlargement of the prostate; and as regards that arising from the opposition to the discharge of the fragments, this can be overcome in great measure by the employment of Sir Philip Crampton's ingenious and scientific contrivance, to which we are glad to find the author specially alludes. Where from the mechanical obstruction offered by the enlarged gland, or atony of the bladder, that organ cannot expel the detritus, Sir Philip Crampton's exhauster is not only a useful but a necessary auxiliary to lithotrity, and no surgeon ought to undertake any such case without being furnished with it.

The *scoop lithotrite* is also a most useful instrument in the cases we allude to; it saves the bladder much trouble, and the patient from the annoyance resulting from the impaction of fragments in the canal; we have seen it used frequently with the happiest effects; still, we cannot but agree with the author that it is "a slow process," and "may do much mischief when rough spiculæ are caught between the jaws."

With respect to the last point considered in this chapter, the "injection of solvent agents into the bladder," the combining "chemical and mechanical forces in destruction of calculus," we have little to comment upon, save to express the opinion that any such measure should be slowly and cautiously undertaken; and that, though the plan appears very feasible, antecedent experience is calculated to raise a strong doubt of its practical utility.

We now take leave of the author, and, in doing so, congratulate him on the successful accomplishment of a difficult and troublesome task. The extracts which have been furnished afford sufficient evidence that the work under consideration is in no respect inferior in merit to that which preceded it from

the same pen on "The Pathology and Treatment of Stricture of the Urethra." There are few achievements, literary or otherwise, that cannot be criticised; and perhaps the present work is somewhat deficient in purely practical matter; but the author's observations on the normal anatomy of the prostate gland, and especially the characters of its most common morbid change, the senile enlargement, are very valuable, and indicate extensive knowledge and industry on the part of the writer. In short, the work, *as a whole*, surpasses anything that has as yet been compiled on the subject of "the Enlarged Prostate."

La Vaccine, ses Conséquences funestes, Démonstrées par les Faits, les Observations, l'Anatomie Pathologique et l'Arithmétique, &c. Par le Dr. G. C. Villette de Terzé, &c. Paris: G. Baillière. 1857. 8vo, pp. 160.

Parliamentary Report, General Board of Health. Vaccination, by John Simon, F. R. C. S. E. 1857. Blue Book.

Copland's Dictionary of Practical Medicine. Part XVIII. Article, "Vaccination." London: Longmans. 1857.

Let us now turn to the last instalment of Dr. Copland's Dictionary of Practical Medicine (Part xviii.), and consult the article "Vaccination" for the most recent testimony relative to the vaccine. We find here an excellent history of vaccination; and every question in connexion with it narrated in a succinct manner. Yet, so far as regards Dr. Copland's views, relative to the prophylactic powers of vaccine lymph, we confess ourselves quite at a loss to discover them. Thus we are deprived of the advantage of an opinion for which we have always entertained the highest respect.

At section 10 he informs us that—

"*There can be no doubt* that vaccination, satisfactorily performed, with recent lymph of a *proper* description—the vesicles having regularly maturated, without hindrance or accident, and with a sufficient number of punctures—*is a most valuable protection from small-pox;* and if this protection be not afforded during the whole life of the individual vaccinated, in some cases, it at least, *in all, is a protection for many years*,—the variolous disease, when caught subsequently, notwithstanding *this protection, being, with few exceptions, a comparatively mild disease,—these exceptions, especially when they are fatal, still admitting of doubts as to the proper performance of vaccination, and as to the efficacy of the lymph or matters employed.* It cannot, however, be denied, that the protection from small-pox, furnished

by vaccination to persons who have been vaccinated in infancy, and have grown up, is not so fully manifested when small-pox is epidemic as at other times; nor does it appear to be as complete when vaccinated persons migrate to warm climates, and are there exposed to small-pox infection, as when they remain in temperate countries; but this requires further inquiry."

Now here we have, in a few words, clearly and in a most decided manner, detailed the amount of protection against variola afforded by successful vaccination; and in these sensible remarks we concur almost to the letter, especially as regards the question of modification of influence upon migration to different temperatures, which, with him, we consider "requires further inquiry." After reading the preceding statement, then, we must confess we were surprised to meet the following one a little further on:—

"Protective Influence of Cow-pox.—This subject, too, occasioned much discussion during the last quarter of a century, and more especially during the last few years; *and the amount of protection which cow-pox affords against small-pox has been often entertained, but, as yet, by no means satisfactorily ascertained.*"

Thus, then, though we are actually in the dark as to what Dr. Copland's real estimate of the value of vaccination may be, yet we can draw a pretty correct inference as to the conclusions to which a student would arrive after the careful perusal of the author's essay. Judging from the opinion to which we should come, had we not already formed one from other sources, it would be, that the whole matter was a humbug. It is not uncommon for compilers of what are termed Medical and Surgical Dictionaries, to fall into a habit of incertitude when dealing with their different subjects, and it is on this account, chiefly, that such publications are the most inappropriate as students' class-books.

The efficacy of perfect vaccination as a protection from small-pox has been proved beyond all doubt. It has been clearly ascertained that if the insusceptibility to variolous contagion has not been rendered permanent by vaccine, that insusceptibility can be renewed, or, if weakened, strengthened, by secondary vaccine inoculation. No one will deny that occasionally small-pox attacks those who have been successfully vaccinated, even before time could possibly have been said to have weakened its powers; yet these are such rare exceptions that they prove the rule. Where such examples do occur, the mortality is very small, and the attack is generally slight. The

process is harmless to the constitution and the *public*, provided the lymph be engrafted by a competent person in such a system as that person would not hesitate to deposit it. That such are among the blessings conferred by vaccination upon the community, every Government concedes, and every physician and surgeon, capable of forming a judgment upon the subject, whose intellect is not warped, or who does not seek to obtain what we would consider an unenviable notoriety, like M. V. de Terzé, yields assent. Consequently, that the practice should be enforced by law, so as to make it universal *in this kingdom at large*, no right-thinking person will gainsay.

A measure to enforce vaccination has been already adopted, but it refers only to England and Wales. Ireland has not been included in its scheme; and, so far as its application to the two former portions of the United Kingdom is concerned, it has been found inefficient and defective.

The defects, &c., of the compulsory Act it is not our purpose to enter into here, especially as they have been so clearly laid down in the memorial presented to the President of the Board of Health in 1855 (Sir B. Hall), by the President and Council of the Epidemiological Society. A few words relative to the system of vaccination as adopted in Ireland we must be permitted to write, which, though there is no compulsory Act with us, is, as in England, chiefly under the control of the Poor Law Commissioners.

Nothing can or does tend more to depreciate the estimation of vaccine inoculation, as a protective against small-pox amongst the public, than the frequency of examples of variola amongst those who are *reputed* as vaccinated. Frequent examples of this nature occur amongst the rural districts of Ireland, and thus the people lose faith in vaccination, and are thrown back upon variolous inoculation. Hence the frequency of a burst of small-pox here and there throughout this country, originating from the niduses formed by itinerant inoculators, who of late have, among us, become so numerous. Now we are under the impression that a very good reason can be assigned for the frequency of the *reputed failures of vaccination as a prophylactic*. A glance at the fifth (the last) "Annual Report of the Commissioners for Administering the Laws for the Relief of the Poor in Ireland," under the Medical Charities Act, throws some light on this question. At page 81 we find a summary of the numbers vaccinated throughout the four provinces, and a glance at the "total" shows that there were 4046 individuals vaccinated in the poor-houses, all of whom, having been constantly under the eye of their respective medical officers, were

duly watched through the stages of vaccinia; and we find so many as 3610 *reported* as "successful cases," leaving a balance of but 436, which, up to the period of the Report, were unsuccessful, and which were, doubtless, re-submitted to vaccine virus. This appears satisfactory, especially as the Commissioners make no mention of small-pox having occurred in the poor-houses.

The number of cases vaccinated by the dispensary physicians, we are informed, was 84,131, but no mention whatever is made as to how many of these were *successfully vaccinated*, and for obvious reasons, because the great majority of them were never seen after the lymph had been inserted. Amongst the "*dispensary patients*" the Commissioners assert, "in certain localities," *small-pox was prevalent* "sufficiently to create public alarm."

We have no hesitation in saying, that numbers of those 84,131, which are said to have been vaccinated, were either really unvaccinated, or, in a state of insecurity, from imperfect vaccination. Yet all these individuals were *supposed* to have been truly vaccinated; and small-pox, if it occurred in the neighbourhood of any of them, and attacked those imperfectly protected, did so to the disparagement of vaccination amongst the immediate public.

It is an established fact, that unless the vaccine runs through its course regularly, the protection afforded cannot be relied on; therefore, those who have been subject to its influence should be inspected at proper intervals, so that if the virus proved abortive, ran an irregular course, or if the vesicles had been injured so as to destroy their characteristics and render the process doubtful, the test should be employed, or the individual re-submitted to infection.

Such a system of inspection cannot be followed up amongst the lower orders, unless some means be employed to—in a degree—enforce the attendance of the parents with their children. In the Cowpock Institution of Dublin the parent is obliged to deposit one shilling, which, upon appearing with the child, or sending the infant with another person on the eighth day, is returned, but if this be neglected the money is forfeited. By this plan, we have been informed, an inspection to the amount of nearly seven-eighths of those vaccinated is secured. Before the Medical Charities Act became law, some description of deposit was generally enforced by the dispensary surgeon, but latterly the Poor Law Commissioners (rightly, doubtless) have forbidden the practice, inasmuch as they consider it illegal; so

that, with every desire to further vaccination, they are constrained to impede its progress.

In conclusion, whatever means may be adopted hereafter to facilitate an universal vaccination, we must confess our entire coincidence with an opinion expressed by the President and Council of the Epidemiological Society in their Memorial to Sir B. Hall:—"It is manifest," they say, "in the first place, that the vaccination of the people, which is a measure undertaken by the State for the security of the public, has nothing in it of the character of alms, and does not fall properly under a department of Government whose sole function is the distribution of alms; while it is equally obvious that it does fall naturally under a department charged with the maintenance of the public health. Had there been such a department in existence when vaccination was first made a matter of public concern, there can be no doubt that the duty would have been placed in their hands, and it is not only proper, but highly politic, *that it should now be transferred to them.* Vaccination, like many other great and beneficial discoveries, has had, and still has, prejudices to encounter; and it is of the last importance that it should be presented for public acceptance in the manner most calculated to soften and subdue these prejudices; *but to stamp it with pauperism*, or to give it even the semblance of an act of poor-law relief, *is not to soften and to subdue, but to aggravate and add to* prejudice."

The Physiology and Treatment of Placenta Prævia. By ROBERT BARNES, M.D. London: Churchill. 1858. 8vo, pp. 208.

ALTHOUGH Dr. Barnes has not hitherto appeared as the author of any distinct or separate obstetric work (except a small one on Uterine Polypus), nevertheless he is a large and able contributor to obstetric literature. At the end of the volume before us is an enumeration of his "works and memoirs;" from this we learn that he is the author of eighteen different communications, which from time to time have appeared chiefly in the pages of the "Lancet," "Medical Gazette," and "Medico-Chirurgical Transactions." All these papers bear the stamp of a vigorous, independent mind; and all are written in language remarkable for strength and clearness. The present work contains Dr. Barnes' Lettsomian Lectures, delivered before the London Medical Society last May, and which are here reprinted without alteration. These Lectures, three in number, occupy 111 pages; the remainder of the book is taken up with

appendices of cases, illustrating the author's views and statements respecting placenta prævia, and with remarks on "the ready method" of Marshall Hall in the treatment of suspended animation of new-born infants.

Our readers must be well aware that the subject selected by Dr. Barnes for these Lectures is one about which a very sharp controversy was carried on, and widely different opinions were entertained. Professor Simpson exhausted his intellectual and statistical resources upon it, and nearly every accoucheur of note gave expression to his opinion respecting it. Still, the prospect of reconcilement seemed as remote as ever. It showed, therefore, not a little courage in Dr. Barnes taking this for his theme. In all controversies there is some truth, we believe, on both sides; but accommodation, or adjustment of conflicting opinions, can never be expected till the facts common to both have been distinctly ascertained: and the work before us does this, we think, and shows how far the dogmas of the contending parties are reconcilable.

In the first Lecture we have a very clear, condensed, historical, and critical survey of the physiology, pathology, and treatment of placental presentation, with remarks upon the causes and diagnosis of this occurrence. Rival claims have been set up in favour both of Levret and Rigby for the first recognition of the difference between hemorrhage from *accidental* detachment of the placenta, and hemorrhage from the *unavoidable* detachment of this organ. After stating that Amand, Lamotte, Guillemeau, Petit, Portal, Giffard, Puzos, Smellie, Rœderer, and Levret, all knew and described, from actual observation, that the placenta, in certain cases, adhered to the lower segment of the uterus; that they all knew and fully comprehended the perilous nature of this complication; that they knew the hemorrhage was inevitable, &c.—he proceeds to say:—

"This current knowledge was first thoroughly digested and systematized by Levret. Did Levret introduce any new fact, propound any new theory, or recommend any new mode of practice? This question must be answered in the negative. Shortly after the publication of Levret's work, the late Dr. Rigby, of Norwich, published the first edition of his celebrated 'Essay on Uterine Hemorrhage.' Up to this time it appears that Dr. Rigby had not seen the work of Levret; and from the terms in which he refers to the views of Mauriceau, Portal, Giffard, Smellie, and others, he does not seem to have been aware of the extent to which his own doctrines had been anticipated. . . . . But since Levret had not 'communicated a new physiological fact,' but had epitomized the current knowledge of his time, confirming it by his own experience, what originality can we

assign to one who in the end claims to share with Levret a merit which Levret does not possess? I say this without any desire to detract from the worth of the writings of these famous authors. The works of both are rich to profusion in the records of facts of imperishable value, and in reflections that will ever prove in the highest degree instructive and suggestive."

The *etiology* of placental presentation receives some notice from Dr. Barnes, and his remarks, though they do not clear up the question, contain, nevertheless, much valuable information. That lateral attachment of the placenta predisposes to its retention, is a fact in which our own experience corroborates that of the author. The diagnosis of placenta prævia, before the occurrence of labour, next comes under consideration. We possess but little knowledge of a definite or precise kind upon this point; and, so far as the medical treatment of the case is concerned, this deficiency is not of much consequence, as at this time preventive or curative measures are not possible, we think. This last opinion may not be universal, for we recollect a medical man once writing to us with a patient, and saying, in reference to her case, "that she was *threatened* with placenta prævia"!

In the second Lecture the author enters upon an exposition of his theory of placenta prævia. The following quotation will convey an idea of his peculiar views respecting the mode of production and of suppression of the hemorrhage which forms so prominent a symptom in this irregularity. After stating that it is the *contraction of the womb*, and not the separation of the placenta, which secures immunity from flooding, he proceeds to say:—

"There is, then, an anatomical, a physiological limit to the extent of placenta liable to detachment during the expansion of the womb. This is why, after a certain stage of the labour, no fresh bleeding surface is exposed. But how is the bleeding stopped from that part of the uterus already bared of placenta? By precisely the same mechanism as that which stops the flooding after normal detachment of the placenta from its normal seat at the fundus. The longitudinal muscular fibres of the lower segment *must contract* to pull open the mouth. Expansion, dilatation of the mouth, is contraction of the cervix. This contraction, by shortening the cervical portion of the womb, casts off the placenta, and exposes the ruptured mouths of the utero-placental vessels. The *first* effect is, bleeding; the *second* is, to stop the bleeding. The contraction goes on either actively, or passively and tonically, in most cases: and this further contraction constricts the orifices of the vessels—closes them: it is hemostatic. If hemorrhage be renewed, it does not proceed, except

under circumstances of excessive muscular relaxation—the 'passive hemorrhage, of Dr. Chowne—from the surface bared by the preceding active contraction: it proceeds from a fresh zone or arc further from the os bared of placenta by another contraction. This zone or arc is, in its turn, in like manner sealed; and there is another pause in the flooding. Zone after zone is thus bared by recurring contractions, and successively sealed up until that physiological limit—that line of demarcation between normal and abnormal placental implantation, the boundary line of placental detachment, which I claim to have discovered—has been reached. *This zone attained, the labour is* A NATURAL LABOUR!"

This line of demarcation, or "lower polar circle," as he elsewhere calls it, is the circumference of the os at its fullest degree of dilatation, and is represented by a circle within the womb, at an equal distance all round, of three inches from the os uteri. All below this line he calls the "cervical zone," or "region of dangerous placental attachment." From the above, and many other passages which might be cited, we gather, first, that, according to the author, *uterine contraction*, though it separates the placenta attached over the os, and thereby causes hemorrhage, is yet the principal mechanical means for its suppression; and, secondly, that when the placenta is separated up to the "lower polar circle," no further spontaneous detachment takes place, and, as a general rule, the hemorrhage ceases, "certain favourable conditions concurring." Based upon these principles, he recommends, in unavoidable hemorrhage, "to separate all that part of the placenta which adheres within the cervical zone or region of dangerous placental seat." This operation can be performed with one or two fingers, and is, therefore, feasible under the precise circumstances which preclude turning, or total detachment. Contrasted with the operation of totally detaching the placenta (so strongly advocated by Dr. Radford and Professor Simpson), it has the further advantage of not endangering the life of the child,—at least it gives it some chance of its being preserved.

Upon the new physiology and practice here put forward we have only a few remarks to offer. These we state with all candour, and with becoming diffidence, having little direct experience of our own to guide us. That the placenta is spontaneously detached only so far as the lower polar circle, seems to be unquestionable in cases of its partial presentation: but we have some doubts whether the same rule applies to cases of complete implantation: the occasional expulsion of the entire placenta before the child would rather argue against it, we think. And it must be remembered that, in a large proportion

of the cases of unavoidable hemorrhage, it is only the *edge* of the placenta that presents; thus, out of *nineteen* cases of placenta prævia, recorded by Drs. Collins, and Hardy and M'Clintock, *ten* were examples of partial presentation of the placenta. In such cases we feel it is doubtful whether the ordinary mode of treatment (viz. by rupturing the membranes and exciting pains) should give place to any other. If uterine contraction follow the rupture of the membranes, the arrest of the flooding is certain; and without uterine contraction, artificial separation of the placenta cannot be relied on; for "this detachment will not of *itself* stop the hemorrhage." The very case which suggested to the author's mind the views here propounded was one of this kind, in which, after plugging, he ruptured the membranes. How far the detachment of the cervical portion of the placenta, in cases of central implantation, will succeed in arresting the hemorrhage, is a question which, in our judgment, yet remains to be decided by experience. If the uterus be in a state of atony, it would, on the principles laid down, be an unsafe proceeding, and likely to augment the hemorrhage; but if active pains were present, we can conceive that it might hasten the dilatation of the os, if not arrest the flooding.

We concur with the author in his strictures upon the operation of totally detaching the placenta, and fully assent to his conclusions, that "it cannot be relied on to arrest the hemorrhage, nor admitted as a principle of treatment of placenta prævia." When discussing this subject he makes some excellent observations on the abuse of statistics. From these observations we cannot forbear making one or two extracts. He is speaking in reference to the operation of artificial total detachment of the placenta:—

"In approaching this question I must, at the outset, declare my conviction that it is not to be decided by statistics. I believe this opinion is now generally entertained by the profession. The statistics, at least, that have hitherto been put forward have satisfied no one. We must not give up physiological reasoning, or individual clinical experience, to a mass of figures which profess to be the expression of facts similar in nature. From the moment that all the details of a case are eliminated, in order to squeeze out one or two particular facts to form part of a statistical phalanx, we lose almost everything that is instructive in the case; and run the most imminent danger of falling into false conclusions. We all know that a single pregnant instance of a fact well observed is often absolutely decisive as to a physiological or pathological law. There are individual facts against which no array of statistics can avail. I say this without any desire to depreciate the value of the application of the

numerical method to medical inquiries, but rather with a view to exact the most scrupulous care in our appeals to this method."

And again he observes:—

"I need but repeat that things different in nature are added together, compared, and made the foundation for practical conclusions. The practice of gathering together, from many different writings, cases presenting some general features of likeness, which have been observed from different points of view, through many different media, and, therefore probably differing in many essential points, must necessarily be unfitted for scientific comparison. No after-analysis, no after-synthesis, can compensate for the want of original essential details."

One of the appendices to these Lectures contains some observations upon Marshall Hall's "ready method," as applied to the restoration of still-born children. Of its utility in these cases the author speaks most encouragingly. He gives, too, an instructive tabular summary of thirty-five cases, in which it was employed with complete success in all but one instance.

We must now bring this notice to a conclusion. Although we have given only a qualified adhesion to the original views which it was the great object of these Lectures to develop, still we have perused them with great profit and satisfaction. They really contain a vast deal of instructive and suggestive matter, relating, not merely to uterine hemorrhage, but to other topics of deep interest to the accoucheur. Dr. Barnes has worked up his subject with great ability and research, and has strengthened his positions in such a way as to render them almost impregnable. To our mind the fact of greatest practical importance established in these Lectures is, the influence of *uterine contraction* as an agency in the suppression of unavoidable hemorrhage. The due recognition of this physiological law cannot fail to affect the treatment of these cases, and to inspire the practitioner with a just and rational confidence in the resources of nature. We have no hesitation in asserting that this work, in addition to its many other merits, goes farther to settle the much vexed question about the treatment of placenta prævia than all that has been hitherto written or said throughout the whole course of the controversy.

On Plastic Operations for the Restoration of the Lower Lip, &c. By Thomas Teale, Surgeon to the Leeds General Infirmary. (Reprinted from the Medical Times and Gazette.) Pamphlet.

A Sketch of the Principles and Practice of Subcutaneous Surgery; being the Oration delivered before the Medical Society of London, March 9, 1857. By William Adams, F.R.C.S., &c. London: Churchill, 1858. 8vo, pp. 67.

We have classed these two brochures together,—although, from their titles, they would seem to have little in common,—because their authors have brought forward, under their respective heads, some of the most interesting additions which have been made to operative surgery in modern times. If the great triumphs of conservative surgery have their attractions for the few whose ambition it may be to be great operators, there are other less brilliant, but equally useful results of modern improvement, which should command the attention of greater numbers among us, because they, or some of them, are within the reach of all surgeons of moderate experience. The remark is trite enough, though constantly forgotten by students, and not always remembered by the practitioner, that the good surgeon is known by the perfection of minor, rather than by the brilliancy of great operations. It is by a knowledge of the fundamental laws of health and disease, and by constant reflection on, and practice of, the minute details of the surgical art, that successful results are to be obtained; and it is because we are impressed with the importance of these facts that we feel called upon to notice, with much approval, the pamphlets at the head of this article. In Mr. Teale's we have the results of sound principles and much thought applied to the successful removal of one of the constant opprobria of surgery—the distortions produced by burns; and in Mr. Adams' we have attention drawn to numerous results of one of the great laws of life, first drawn forth to light by John Hunter. Both papers bear the impress of men who go to work in the right way, and who are not to be classed with the herd of scribblers who flood the pages of our medical literature with so much that is wearisome to read and useless to remember.

Mr. Teale, whose name is favourably known in connexion with the Leeds Infirmary, has devised some useful improvements in plastic surgery of the lips and face. The cases which he publishes are six in number, and comprise operations for the restoration of the lips, for the removal of ectropion, and for

the lessening of other distortions produced by burns. The cases are illustrated by drawings before and after operation, the latter being sometimes taken as many as three years after the wound had healed, and so giving a very fair idea of the probable amount of permanent improvement. Mr. Teale has followed the plan of dividing through the cicatrix, probably in ignorance of the valuable improvement due to Celsus originally, and revived by Pancoast in America, and Crampton and Rynd in Dublin, of cutting wide of the cicatrix in sound skin. He has, however, adopted Pancoast's other great improvement, viz., the interposition of a piece of sound skin between the edges of the wound on the raw surface obtained by division of the cicatrix. The combination of both plans has had good results in one case that came under our notice in this city. Generally, however, we must admit, that the improvement has neither been so permanent nor so complete in those cases that we have seen, as would appear to have been obtained by Mr. Teale. The general plan which he has adopted for deformities of the neck has been already referred to, as resembling the ordinary modes of dividing the cicatrix, dissecting down to sound areolar tissue, and interposing as sound a piece of skin as can be obtained without stretching; he advises that too much pains should not be spent on making the flap meet in all points the edges of the wound, but to be satisfied with getting one border to unite by first intention, if, indeed, we can secure even that much. The novel feature in this pamphlet is the plan of forming new lips, at once ingenious and effectual. The lower lip is formed in the following way:—

"Two vertical incisions, about three-quarters of an inch in extent, are made through the everted lip down to the bone. These incisions are so placed as to divide the upper portion of the everted lip into three parts, the middle being equal to one-half of the natural breadth of the lip, while the two lateral portions are equal to one-fourth. From the lower end of each vertical incision the knife is carried in a curving direction outwards and upwards, to a point situated about one inch from the angle of the mouth opposite to the second molar tooth of the upper jaw. The two flaps thus marked out and deeply incised are then separated from the bone, the mucous membrane uniting them to the alveoli being freely divided. Lastly, a bare surface is made along the alveolar border of the middle portion of the everted lip. The incisions being now completed, the lateral flaps are drawn upwards and united by twisted sutures to each other in the median line, and to the middle portion of the everted lip at their inferior border. In this way a new lip is, as it were, built upon the middle portion of the old one."

The upper lip is restored, in cases where only a narrow band has been left, by making a crucial incision across the lip and cheeks (*en saltire*) enclosing acutely angular flaps, and dovetailing these flaps, thus increasing the depth of the lip at the expense of its width. For further details we must refer to the pamphlet itself, which, as we already said, is copiously illustrated; Mr. Teale's cases are modestly detailed in a few words, and are evidently published for the benefit of the members of the profession, to whose notice we heartily commend it.

Mr. Adams' lecture has for its object the elucidation of Hunter's great law of the reparative process in the two classes of injuries which may be termed open and subcutaneous wounds. "Injuries in which the parts do not communicate externally do not inflame, while those which are open commonly both inflame and suppurate." In this law of Hunter's the whole principle of subcutaneous surgery is embodied. Mr. Paget has investigated the processes which go on in these two classes of cases, and has shown the difference in the mode of organization of the lymph, which is the basis of repair in each case. In the more rapidly effected organization the connecting tissue is formed without the intervention of cells, while in the granulating process of open wounds cells have to be formed and converted into fibre; this latter process partakes, in fact, of the nature of inflammation,—the lymph or plasma which is thrown out on open surfaces approaches more or less in the forms it assumes to inflammatory exudation. We can trace the gradual deterioration of lymph from the healthy nucleated blastema of closed wounds through the fibre-cell and granular cell of open wounds, down to the imperfect cell known as the pus corpuscle. These gradations we have often traced in the course of microscopic investigations: they are clearly shaped out by Mr. Paget in his valuable Lectures on Inflammation, and receive further confirmation in the interesting Address of Mr. Adams. The greater part of the Address is taken up with practical results of the working of this law, or, to speak more clearly, with examples of the advantage of employing subcutaneous surgery in preference to open wounds as far as possible. It will suffice to name the subcutaneous division of tendons substituted by Stromeyer in 1830, for the open section of Thilenius in 1782 and of Delpech in 1816; the removal of loose cartilages from the knee-joint by subcutaneous section, as performed by Goyrand of Aix in 1840, and a few months later by Mr. Syme; the subcutaneous division of inflamed periosteum and of varicose veins. Add to these the advantages of the early application of

the starched apparatus in compound fractures, a plan of treatment we desire to see more extensively applied in this country, and which has produced astonishing effects in the hands of those who have tried it; and we think we have given examples enough to show that the principle of removing wounds from the action of the air is not only a sound fundamental principle in surgery, but one that admits of much wider application than to the section of tendons and the treatment of burns. We have not alluded to many other cases in which it will be found useful, because we desire that the book itself should find its way to the libraries of all practising surgeons, whether their spheres of action be wide or narrow. Those who read it will pardon a little pomposity of style almost inseparable from the orational form, for the sake of the many suggestions it will afford for practice. It is a very useful study to follow out the application of a sound law such as Hunter's, and to connect by analogies, that really exist, cases that at first sight present no points of similarity; the exercise is one that enlarges the mind, and is the fruitful parent of many an improvement in practice, as well as of most advances in science.

A Treatise on the Employment of the Speculum in the Diagnosis and Treatment of Uterine Diseases; with 300 *Cases.* By Robert Lee, M.D., F.R.C.S., &c. London: Churchill. 1858. 8vo, pp. 132.

"Progression by antagonism" seems to be a law in medicine as well as in other sciences. Great discoveries have always been met by sturdy opposition. The great Harvey was called a "Sadducee." Vaccination called forth the bitterest antagonism; and "Laennec's trumpet" we can remember as a term of ridicule. Yet the circulation of the blood and vaccination have become indubitable facts, and no one now would prescribe for chest diseases without using the stethoscope, whether he understand what it reveals or not. Something of the same process is now going on about the speculum uteri; the one party crying it up as universally necessary and invariably useful; the other pouring out the vials of their wrath upon the worthless tube. As we have repeatedly noticed the works of the advocates of the instrument, it is only fair to notice one of its opponents, and on this ground we welcome Dr. R. Lee's book. We did not expect a calm, impartial judgment from Dr. Lee; he is too one-sided for that; and moreover he had taken too decided a part early in the discussion to leave a hope that any amount of experience would change his opinion.

Nevertheless, as we think the time has come for an attempt at fixing the precise value of the speculum, we willingly make use of Dr. Lee's book as our text in the few observations we have to offer. But before entering upon the subject, on which we shall venture to differ from Dr. Lee, it will be more pleasant to notice one or two subjects on which we more or less agree with him.

1. The author has given details of 300 cases in which the use of the speculum was unnecessary or injurious. His summary is as follows:—

"Of these 300 patients, 47 were unmarried: one had barely completed her eighteenth year; there were several under twenty, and the greater number under thirty years of age, and were suffering from hysteria, leucorrhœa, dysmenorrhœa, or some nervous affection of the uterus, without inflammation, ulceration, or any structural disease or displacement of the organ. In Case 256 the patient had been informed that the womb was prolapsed and much ulcerated, and some instrument had been introduced daily for six weeks, by a physician extensively engaged in the treatment of uterine disease, and great expense incurred, with an aggravation of all the symptoms. In this case I found the hymen so perfect that it was impossible to reach the os uteri without employing an unjustifiable degree of violence. On the ground of morality, and on every other ground, the employment of the speculum in these 47 cases could admit of no defence. Of these 300 patients, 70 were barren, and the sterility was not removed, nor the hysteria, leucorrhœa, nor disordered menstruation, under which the greater number were labouring, in a single instance relieved, or any benefit obtained. The injurious effects of a long course of speculum and caustic treatment upon the moral feelings and characters of several individuals were not attempted to be concealed, the treatment being spoken of with horror and shame.

"Of these 300 patients there were a considerable number suffering from cancerous disease of the uterus, in all of which the symptoms were increased by the introduction of the speculum, and the application of caustic or the actual cautery to the ulcerated vagina, and os and cervix uteri. In Case 236, though the carcinomatous ulceration was in an advanced stage, and the nature of the disease obvious to the most inexperienced, after an examination with the speculum, a false prognosis was given, and iron, heated to a white heat in fires of coke, was for months passed through the tube, and delusive hopes of recovery held out to the last, and the pecuniary concerns of the husband involved in irretrievable ruin by the charges, medical and surgical, incurred by such unscientific, unprofessional, and unprincipled proceedings."

We shall not yield to Dr. Lee in our indignation at such shocking proceedings. These cases display an amount of wan-

ton, ignorant quackery without equal. We have ourselves, on a smaller scale, seen something of the kind, when the speculum was used unnecessarily, injuriously, and as a means of extracting money, and we have never scrupled in denouncing such infamy. But, scientifically considered, and judging calmly, these cases only prove that there are ignorant and unprincipled men in the profession; they no more prove that the speculum is necessarily useless or injurious than that criminal abortion produced by ergot of rye is a fatal argument against that drug; or that because strychnia has been used as a poison, medical men ought to banish it from their materia medica. Let us keep steadily in mind that the abuse of an instrument is no argument against its proper use.

2. With the following paragraph we cordially agree:—

"An examination of the physical condition of the uterus in unmarried women, either with or without the speculum, I have always refused to make, unless pain, severe and almost constant, in the uterus, existed, leucorrhœa or hemorrhage, which did not yield to treatment, and when the symptoms did not make me strongly suspect the presence of some displacement or organic disease. In unmarried women, whatever their rank or condition in life may be, the integrity of their structures should not be destroyed with the speculum, nor their modesty wounded by an examination of any kind, without a necessity for such a proceeding being clearly shown. Even in married women, who are barren, or who have had children, it is unjustifiable, on the grounds of propriety and morality, to institute an examination of any sort, unless the symptoms warrant the supposition that the uterus is displaced or in a morbid condition, the nature of which cannot be determined by the symptoms alone. Numerous cases of leucorrhœa in young unmarried females, when rational constitutional and local treatment is adopted, perfectly recover, when no examination is made."

Eight years ago, after some remarks upon the speculum, we concluded by saying that "to make an examination with the finger or speculum, unless it be plainly necessary, is a flagrant breach of delicacy; and in the case of young unmarried females, it is almost a crime."

3. We are inclined to agree with Dr. Lee, that in the zeal of some of the "apostles of the speculum" they have undervalued the information attainable by the finger alone; not, perhaps, in so many words, but practically. Now, we should ourselves rate the value of the knowledge thus to be acquired as highly as that to be obtained by the speculum; and it must be from these conjoined sources that future improvements in diagnosis are to be looked for.

Having made these preliminary remarks, let us now turn to Dr. Lee's statement of the *legitimate* use and *real* value of the speculum in practice. It might indeed be comprised in five words, "nothing, or next to nothing," but we must go a little into detail:—

"1. In the first great class of organic uterine diseases, which comprehends fibrous, fibro-cystic, glandular, and all other tumours which are not malignant, I have derived little or no aid from the speculum in their diagnosis or treatment."

Probably not; but we can confirm (if that could be necessary) the statement of Dr. Montgomery, that a small polypus may be detected within the os uteri *before* it can be felt; and when through the os, it may be more easily removed by torsion with the aid of the speculum than without it.

"2. In all the varieties of malignant disease of the uterus, scirrhus, fungoid cancer, and corroding or phagedenic ulceration, the speculum has given me no assistance whatever in their diagnosis or treatment, either in the early or in the advanced stages."

In this we cannot agree with Dr. Lee. We distinctly remember a case of polypoid growth which resembled to the touch an ordinary polypus, but which the speculum showed to be encephaloid cancer. Again, in doubtful cases of corroding ulcer, the aspect of the surface will be good evidence in forming our opinion. Moreover, by means of the speculum it is more easy to obtain the material for a microscopic examination which may be decisive. Lastly, though we should reprobate as utterly as Dr. Lee the application of the actual cautery, or strong caustics at frequent intervals, in cancer, there are some cases of both cancer and corroding ulcer in which occasional local applications, cautiously made with the speculum, may arrest excessive discharges, and retard the progress of the disease. This we have repeatedly seen. We may admit, however, that it is not in this class of diseases that the advantage of the speculum is greatest.

"3. In cases of obstinate leucorrhœa I have often employed the speculum in married women after I had failed to detect the existence of disease by the ordinary mode of examination. In some of these cases there has been seen an unusual degree of redness of the os uteri, sometimes affecting the whole, and at other times limited to the inner margin, with or without swelling. The white viscid discharge has been seen issuing from the os uteri. I have never seen ulceration of the orifice of the uterus in such a case; and the condition of the interior of the cervix I have never been able to

demonstrate, either with the bivalve or other speculum; nor do I believe that in the ordinary condition of the os uteri it is possible to see the inner surface to any great extent by any means."

The limited local inflammation thus discovered may be easily cured by applications through the speculum, in cases where the general diffusion of the same means in an injection would not be desirable. We cannot see further into a millstone than Dr. Lee, and have never fancied that we could see into the canal of the cervix "to any great extent;" but we have seen the os wide, its lips everted and red, and this redness apparently extending up the canal, indicating, of course, the extension of the inflammation; and as such cases are more common than either fibrous tumour or cancer, the value of their diagnosis by the speculum is great.

"4. Sometimes one or both lips are in the condition which is usually called hypertrophy, and which has no relation to cancer."

The speculum is not necessary for the detection of this form of disease when simple, but when complicated with erosion, eruptions, or fissures, its aid is indispensable, and certainly most valuable for the treament of the simple form. We are no admirers of the potassa fusa, we must candidly confess; but we have found most beneficial results from the application of strong tincture of iodine, which can only be done through the speculum.

"5. At other times both lips are swollen, nodulated, or fissured, and the mucous membrane covering them intensely red, with an appearance of superficial excoriations or granulations, which are elevated above the surrounding surface. These apparent granulations are usually considered and treated as ulcerations of the os and cervix uteri, but they do not present the appearances which ulcers present on the surface of the body, or in the mucous membrane lining the viscera, and they are not identical with the granulations which fill up healthy ulcers."

We think it a great pity that these abrasions were ever called ulcers, for they are little more than excoriations of the mucous membrane, although Dr. Tyler Smith's researches have shown that the destruction is sometimes more profound. And although we admit that real simple ulceration is very rare, we cannot assent to Dr. Lee's denial of its existence, for we have seen and cured several cases of it.

These erosions or abrasions, whether granular or smooth, cannot be detected without the speculum; and many of them cannot be cured except by local applications, which cannot

with certainty or safety be applied without it. Now, if we say that for every patient who suffers from cancer, corroding ulcer, or fibroid tumours, at the very least a dozen or twenty have their comfort destroyed and health impaired by these granular erosions, connected, too, as they are with menstrual derangements, &c., we may form a notion of the utility of the speculum. We do not think it necessary to refer more at length to the proper treatment; but we may repeat, that we object, as strongly as Dr. Lee himself, to the treatment adopted in the cases he relates.

4. To this statement of Dr. Lee's experience, we shall venture to add a few examples in which we have found the speculum most useful either for diagnosis or treatment. We have seen sterility of a temporary character cured by the removal of the disease which caused it, and which disease was both diagnosed and treated by means of the speculum. But the causes of sterility are so various, and many of them so remote and obscure, that to recommend the speculum as a mode of cure would be an absurdity.

5. Cauliflower excrescence, when very small, may be recognised by means of the speculum, when examination by the finger would leave us still in doubt; and after removal by the ligature the origin of the growth may be cauterized through the speculum, and can be done by no other means. By this means we succeeded in completely curing one case.

6. We have in a few cases discovered a distinct pustular eruption upon the cervix by the speculum, after we had in vain endeavoured to make out its nature by the finger; and Dr. Simpson mentions herpetic eruptions discovered by the same means. We have felt also a peculiar granular condition of the cervix, just as if grains of small shot were scattered over it, the nature of which could only be resolved by the speculum.

7. Lately, in examining a case, we found the upper part of the vagina and cervix rough, and as if covered with some substance, which puzzled us much, until, on introducing the speculum, we found an inflamed surface, covered with an exudation like diphtherite, in shreds and patches.

8. The condition of the upper part of the vagina must, in many cases, be doubtful, if we trust to the finger alone; visual examination alone can satisfy our doubts. Then we may discover inflammation, general or in patches, papulæ, pustules, or ulcers.

9. We might very fairly adduce the large class of syphilitic affections, in which we are equally indebted to the use of the speculum for much of the more accurate knowledge we

have acquired; but this subject is not touched upon by Dr. Lee, and therefore we shall only thus refer to it.

10. Lastly, we repeat that one inestimable benefit of the speculum consists in the power it gives us of applying remedies directly to the part affected, and to it only, without injuring neighbouring parts. The substance to be applied, and the propriety of applying it, must rest with the discretion of the practitioner.

We trust that the remarks thus thrown together will be sufficient to prove that there are two sides to the question; that if the advocates of the speculum have overvalued it, Dr. Robert Lee has undervalued it. We have admitted that the speculum may, and on Dr. Lee's evidence has been, atrociously misused; that men have shown themselves dishonest, indecent, and ignorant. We by no means undertake to defend the special means of cure adopted by means of it, nor the repetition and prolongation of such treatment; but, rejecting all such, we maintain that if a man of Dr. Lee's sagacity and opportunity had observed without prejudice, and judged with a calm mind, he would have found many reasons for the seasonable and proper use of the instrument, and many cases in which his diagnosis would have been more correct, and his treatment more efficacious.

On Cough: its Causes, Varieties, and Treatment; with some Practical Remarks on the Use of the Stethoscope as an Aid to Diagnosis. By Robert Hunter Semple, M. D., &c. &c. London: Churchill. 1858. Small 8vo, pp. 174.

This treatise is, as the title-page announces, an essay on the subject of "Cough;" and in it we certainly have this affection, occurring from almost every recognised source, alluded to, more or less briefly.

The author commences by giving a short account of the various organs particularly engaged in the act of respiration; glances at the physiology of this most important vital function; enumerates the greater number of causes which are supposed to give rise to cough; and, after touching upon the pathology of the different varieties, recounts the means of treatment most approved in each. All this, we are free to admit, is done in a fair professional manner. The best authorities are quoted as sources of information, whilst empiricism and panaceas are carefully avoided. But we cannot with truth say that we have been much enlightened by the perusal of these pages: there is

nothing whatever of novelty introduced either as to the etiology, diagnosis, or treatment of cough; and it is needless to observe that many of the subjects which come under our author's notice can receive but a passing attention in the space at his command.

The book, in reality, is not likely to be of value to the *professional*, but, attracted by its title, the *general* reader may glean some scraps of information from it.

Nouvelle Comparaison des Membres Pelviens et Thoraciques chez l'Homme et chez les Mammifères, deduite de la torsion de l'Humerus. Par CHARLES MARTINS, Prof. d'Hist. Nat. Med. de la Faculté de Medecine de Montpellier. Montpellier: 1857. 4to, pp. 75.

THIS *brochure* is reprinted from the Memoirs of the Academy of Sciences and Literature of Montpellier, for the year 1857. The author appears to have been occupied during many years in endeavouring to work the problem of the parallelism of the anterior and posterior limbs, having applied himself first to the special study of this subject in the year 1827, along with his friend, Dr. Coindet, of Geneva. Before we come to consider the peculiar views which M. Martins has put forward in this ingenious treatise, it may be necessary to enter into some historical details, for the purpose of laying before our readers a brief sketch of the various different phases under which a matter, apparently of great simplicity, may be and has been viewed by some of the greatest anatomists of modern times.

Vicq D'Azyr was the first anatomist who seriously discussed the problem in question. Having placed the bones of a superior extremity beside those of the inferior of the corresponding side, for the purpose of comparison, and perceiving at once that a difficulty arose from the very evident fact that the flexion of the knee and elbow-joint respectively was in an opposite direction, he hit upon the somewhat strange idea of comparing the right inferior extremity, viewed anteriorly, with the left superior, viewed posteriorly; for, in this parallel, the femur and the humerus have their superior articular heads turned alike inwards towards the spine. Having detailed the comparison of these two bones, he explained the difference in the structure of the forearm and leg by their adaptation to different functions. According to this anatomist the tibia represents the ulna; the fibula, the radius; the patella, the olecranon; yet, strangely enough, as M. Martins remarks, after he

had failed to recognise the analogy of the radius to the tibia in man, he established it for the ruminants, among which, he says, "the ulna is the shortest bone of the forearm, a true styloid bone, terminated by a large apophysis; the fibula," he adds, "resembles exactly a styloid bone; the forearm and the leg are formed by two very considerable bones, which are the *radius* and the *tibia*."

The author proceeds to show that this explanation of Vicq D'Azyr is not admissible, for, even allowing that there is a certain degree of parallelism between the posterior view of the left arm and the anterior of the right leg, the ulna corresponding to the tibia, and the radius to the fibula, the parallelism of the hand with the foot is not preserved, inasmuch as the thumb is turned outwards, and the great toe inwards.

Sömmering has touched but lightly on the subject in his great work[a], and has merely alluded to such resemblances as were evident and incontestible.

From many passages throughout Göthe's "Studies of Philosophical Anatomy," it would appear that this writer recognised the analogy of the anterior with the posterior limbs; he considers the radius to be the representative of the tibia, the principal bone of the leg; he looks upon the fibula and the ulna as merely accessory bones. These views were not published until 1817, two and twenty years after they were written.

Meckel, in 1816, compared the olecranon to the patella, the ulna to the tibia, the radius to the fibula. He remarked that pronation appears to be the normal condition of the anterior limb among animals, without saying whether he considered the lower extremity in man as a thoracic limb in a state of pronation.

About two years later De Blainville endeavoured briefly to establish the view that the radius is the representative of the tibia, and that the patella replaces, as it were, the olecranon.

In the year 1824 Dr. Barclay published his large work on the Bones of the Human Body. This writer also considered the tibia as the analogue of the radius, the ulna that of the fibula, being, like it, a variable bone, sometimes large, and sometimes small; in fact, he says, in some cases the fibula forms part of the articulation of the knee, while the ulna does not enter into that of the elbow; again, as in man, the fibula does not articulate with the femur, while the ulna contributes to the formation of the elbow-joint.

[a] De Corporis Humani Fabricâ. 1794.

Gerdy, who wrote in 1829[a], admits that the patella corresponds to the olecranon, but considers the attachment of the former to the tibia as an *anomaly*. He takes no notice of the labours of preceding writers, and does not appear to have solved any of the difficulties indicated by the several anatomists whom we have previously mentioned.

Frederic Blandin[b] returned to the explanation of Vicq D'Azyr, and endeavoured to establish these views by new arguments; he assigns three reasons for maintaining that the tibia corresponds to the ulna:—1st. Because its head articulates with the femur, as that of the ulna with the humerus; 2ndly. Because the ulna is adapted inferiorly to the cuneiform bone, the analogue, according to him, of the astragalus; and 3rdly. Because the crural triceps is inserted into the patella, as the brachial is into the ulna, while the common flexor of the fingers is similarly related to that bone, as that of the toes is to the tibia. Having established the analogy between the ulna and the tibia, Blandin considered it useless to show the similarity of the fibula to the radius. He remarked that the brachial biceps is attached to the last bone, as the crural is to the fibula; the long flexor of the thumb to the radius also, as that of the great toe to the fibula. He agrees with Gerdy in explaining the differences of the two limbs by the difference in their functional adaptations.

Bourgery, in instituting a comparison between the superior and inferior limbs, lays considerable stress on the functions which they have respectively to fulfil. While he recognises the similarity of the posterior aspect of the arm and the anterior of the thigh, he compares, with Vicq D'Azyr, the humerus of one side to the femurs of the opposite. His treatise abounds in contradictions, and yet he appears to have been the first to have clearly assigned to the tibia an ulnar character (so to speak) at its upper or femoral end, and a radial at its inferior or tarsal.

Cruveilhier, having compared the upper with the lower extremity, drew the following conclusions:—1. That neither of the bones of the leg represents, *per se*, one of the bones of the forearm. 2. That in each of the leg bones we find characters assimilating, some to those of the ulna, and some to those of the radius. 3. That, pronation being the natural position of the forearm, and the leg also being permanently pronated, we should not compare a supinated forearm with the leg.

[a] Sur le Parallèle des Os.
[b] Nouveaux Elements d'Anatomie Descriptive. 1838.

This, as the author remarks, is merely the hypothesis of Bourgery reduced to a more definite formula; and although most authors attribute this explanation to Cruveilhier, under the designation of *hypothèse du croisement*, scientific justice requires that the merit should be assigned to Bourgery, who announced his views in 1832.

In 1838 M. Flourens applied himself to an examination of this subject. This anatomist compares the leg with the pronated forearm of the same side; in this case, then, it is evident that the bones of the forearm are crossed.

In fine, the several views of the parallelism of the superior and inferior extremities, according to the preceding theories, may be reduced to the following three, viz:—

1. Vicq D'Azyr's view: in which the upper extremity of one side is compared with the lower of the opposite.

2. The parallel detailed by Bourgery, combining the hypothesis of Vicq D'Azyr with the "croisement," in virtue of which the head of the tibia represents the ulna; its inferior half, the radius; while the femoral extremity of the fibula corresponds to the radius; its tarsal end, to the ulna.

3. The explanation of M. Flourens, in which the pelvic limb is assimilated to the corresponding thoracic one, the forearm being in a condition of pronation.

Having now endeavoured to give a condensed analysis of the historical part of the work, we proceed to lay before our readers a succinct resumé of the *hypothesis of humeral torsion*, as proposed by M. Martins in the essay before us. This author conceives that to each of the comparisons already examined serious objections may be made; and finding that anatomists in the present day are not quite agreed upon what he considers the most essential point in the comparison, namely, the identification of the two bones of the leg with those of the forearm, he has proposed to consider the humerus as being twisted on its axis through 180°; in fact, a twisted femur. He imagines, therefore, that before we can compare the two limbs, we should first suppose the humerus to be *untwisted* on its axis so as to bring the flexure of the elbow-joint to the posterior aspect. "This done, the comparison of the pelvic and thoracic extremities presents no difficulty."

To do justice to the many ingenious arguments adduced by the author from the wide field of comparative anatomy, we should require the aid of illustrative diagrams; we must therefore be content with giving a short epitome of his theory, referring our readers in the meantime to the essay itself for more detailed information.

The humerus is a bone twisted on its axis. The amount of this torsion is 180° in terrestrial and aquatic Mammalia; 90° in Cheiroptera, birds, and reptiles. In man and the anthropomorphous monkeys, the neck of the bone does not participate in the torsion of its shaft. This neck is twisted to 90° in terrestrial and aquatic Mammalia. Consequently, in man, and the anthropomorphous monkeys, the axis of the neck, that of the shaft, and that of the trochlea, are in one and *the same plane;* in the other Vertebrata, the axis of the neck and that of the shaft are in a plane *perpendicular* to the axis of the trochlea. This torsion is virtual: it is never effected mechanically, any more than the inversion of certain corollæ, and the rotation of hemitropous crystals. In order to compare the humerus with the femur, which latter is a straight bone, we must untwist it through 180°; by such means only can we reduce the thoracic limb to its type of a pelvic limb. The necks, the condyles of the two bones, are now directed alike respectively; the patella and the olecranon are in front; the tibia and radius, the great toe and the thumb, inwards; the fibula and ulna, the little toe and little finger, outwards.

Tibia and Fibula.—The femoral head of the tibia in monodelphous Mammalia is formed by the coalescence of the humeral heads of the radius and ulna; the external articular face of the tibia corresponds to that of the ulna, the internal to that of the radius. The patella, homologous to the olecranon, is attached to the crest of the tibia, homologous to the posterior crest of the ulna. The superior third of the fibula is represented by the anterior or coronoid half of the ulna. In some Marsupials, e. g. Phascolomys, Phalanger, Dasyurus, and Didelphis, the coalescence does not take place; moreover, in these animals the patella is attached to the fibula. In the Monotremata (Echidna, Ornithorhyncus) a tibial patella and a fibular olecranon correspond to the double olecranon of the ulna. In all the Mammalia the two inferior thirds of the tibia represent the corresponding portion of the radius; the two inferior thirds of the fibula, the same part of the ulna. The olecranon is a process which is characteristic of the terrestrial Mammalia and the Amphibia: it is wanting in those mammals which are exclusively aërial or aquatic.

Pelvis and Shoulder.—The symmetry of the human body, or rather the repetition of the parts of the skeleton in relation to a horizontal plane passing through the umbilicus, does not become evident until both arms are elevated above the head: the latter then corresponds to the sacrum, the shoulder to the iliac bones, the thoracic limbs to the pelvic, the ribs to the in-

tersections of the rectus muscle, &c. In order to recognise the homologue of the shoulder in the pelvis, we must then compare an ilium to the symmetric image of the shoulder of the same side, such as we see it in a mirror placed under the real shoulder: this amounts geometrically to a comparison of this ilium to the reversed shoulder of the opposite side. The crest of the ilium corresponds to the scapular border of the blade-bone; the external iliac fossa to the supraspinous; the internal iliac to the subscapular; the ischium to the coracoid process; the pubis to the clavicle. The scapular crest and the acromion have no parts in the pelvis corresponding to them; they are likewise abortive in the Cetacea.

Muscles of the Thoracic and Abdominal Limbs in Man.—Amongst these the author distinguishes:—1st. The *homologous* muscles, both of whose extremities are attached to homologous osseous points. 2ndly. *Analogous* muscles, of which one attachment only, usually the peripheral or distal one, is homologous. 3rdly. Muscles whose analogues have not yet been recognised.

In man, the head of the humerus not participating in the torsion of the shaft, the homologous and analogous muscles of the shoulder and the pelvis have the same position, the same direction, the same functions, thus,—the homologous muscles are, glutæus medius and supraspinatus; glutæus minimus and infraspinatus. The analogous muscles are, glutæus maximus and scapular deltoid.

On the thigh, the rotation of the tibial end of the femur through 180°, corresponding to the cubital extremity of the humerus, has removed to the front the muscles of the posterior plane, e. g. the crural triceps and the brachial triceps; short portion of femoral biceps, and brachialis anticus, &c. On the forearm the same inversions: the external muscles of the leg become internal, the posterior anterior, e. g. the outer gastrocnemius and the flexor carpi ulnaris.

Arteries and Nerves.—A similar torsion brings to the front of the limb the popliteal artery, which is the homologue of the brachial in the upper extremity; the ulnar vessel corresponds to the fibular, the radial to the posterior tibial. Now, if upon a femur we imitate with threads the nervous system of the thigh, viz., the crural, the sciatic, the external and internal popliteal, and then turn the bone through 180°, we shall have represented the nervous system of the arm. The crural, having become radial, surrounds the bone following the plane of extension; the sciatic and the external and internal popliteal, becoming anterior, represent the ulnar and median respectively.

It will appear, then, from the above outline of the ingenious and novel hypothesis of M. Martins, that this view of the muscular, arterial, and nervous systems of the arm and forearm tend to strengthen the idea of a torsion of the humerus, for all these structures are disposed as they would be on a femur whose condyles may be supposed to have suffered a rotation of 180°, while the head of this bone is allowed to retain its normal position in relation to the cotyloid cavity. The functional adaptations would be quite insufficient to account for the differences which have been shown to exist between the thoracic and abdominal limbs; these being the result of higher organic laws, by which they are controlled and determined.

On Squinting, Paralytic Affections of the Eye, and certain forms of Impaired Vision. By CARSTON HOLTHOUSE, F.R.C.S.E., Surgeon to the Westminster Hospital, &c. &c. London: Churchill. 1858. 12mo, pp. 210.

SOME nineteen years since the first successful case of operation for the cure of strabismus was brought under the notice of the profession by Dieffenbach; since that time thousands of squint cases have been operated on, and volumes, in the form of treatises, monographs, and "cases," have been published on the subject; still, it seems that more was required to be written on the matter, and hence we have the little work of Mr. Holthouse submitted to the profession, which, it appears, "is still divided on many points essential to the right treatment of the deformity."

"One ophthalmologist asserts," he says, "that in almost every case of strabismus both eyes are equally implicated in the abnormal position: another holds a directly opposite opinion. The former maintains that it is immaterial which eye is operated on: the latter considers it essential to determine which is the really affected eye and to operate on this only. There are others again who consider the deformity to depend upon an imperfection of vision, and say that if this be remedied no operation will be required, for the eye will right itself. Such contrariety of opinion is not flattering to medical science, and proves how much yet remains to be elucidated."

With the latter part of this sentiment we quite agree, for much still requires to be learned upon every subject connected with medical science; but we cannot subscribe to the doctrine that differences of opinion on scientific subjects can

be regarded as not very flattering to the science itself, though the professors of these contrarieties are not all in a very flattering position, for it is only by thorough investigation that any such matter can be rendered useful, and made an irrefragable truth; and hence differences of opinion are not only of essential value in the elucidation of facts, but also confer on the question an amount of importance it would not otherwise possess. Now as regards the nature and treatment of squinting, we are aware that great good has arisen from the discussions which the question has engendered, and we are are not quite sure that the most recent views which have been put forth respecting it will settle either its pathology or the peculiar operation for the cure of the deformity in every instance. There is no doubt that the author of the treatise before us has brought considerable experience and observation to bear upon the pathology and treatment of squinting; but as he is at issue on several points with gentlemen who have had equal opportunities of making observations, we may presume that his doctrines will be put to the test, and that the present volume will not be the last, touching the improvement of "the human face divine"! After explaining the different varieties of convergent and divergent strabismus, and illustrating these by cases, Mr. Holthouse enters upon the question of the pathology of the deformity, and discusses it with clearness and originality. He mentions three exciting causes as ascertained, stating that there are cases which may be classified as of "uncertain origin." The three ascertained are, strabismus depending upon "nervous origin," "inflammatory origin," and of "hypertrophic origin;" and he states that:—

"It would appear that strabismus may be associated with the following muscular conditions, viz.:—

"1. Simple passive contraction, in consequence of paralysis of an antagonist.

"2. Hypertrophy, with or without structural shortening.

"3. Structural shortening. An arrangement of these," he continues, "according to their supposed cause and order of sequence may be made thus:—

"*Paralytic Cases.*

"1. Simple passive contraction of a healthy muscle from paralysis of an antagonist. This may be followed after a time by—

"2. Hypertrophy of the passively contracted muscle. After a longer interval, it seems probable that a further change may take place; one, namely, of—

"3. Structural shortening and thickening of the contracted

muscle, with degeneration of its tissue. A still further change seems not improbable; one, namely, of—

"4. Complete atrophy of the shortened and degenerated muscle."

Again:—

"*Non-Paralytic Cases.*

"1. Simple hypertrophy from over use, the antagonist being healthy.

"2. Hypertrophy with structural shortening, the result of usually violent spasmodic action, the antagonist being healthy."

Then, after fully explaining his views, and the facts and arguments he has brought forward to illustrate the pathology of strabismus, he considers himself warranted in drawing the following deductions:—

"1. The most frequent *exciting* cause of strabismus is some lesion of the nervous centres or nerves: and next in frequency are inflammatory affections of the eyes.

"2. The *essential* or *immediate cause* of confirmed non-paralytic strabismus is shortening, with or without hypertrophy: or simple hypertrophy of the orbital muscle in the direction of which the eye is drawn.

"3. These muscular changes may be associated with thickening and contraction of the conjunctiva and sub-conjunctival tissue, and an adhesion to the sclerotic coat of the eye.

"4. The above-named changes may affect both eyes, though they are more commonly confined to one eye; in the former case it is immaterial which eye is operated on: in the latter it is not immaterial, but on the contrary essential, that the affected eye should be distinguished, and selected for the operation.

"5. The imperfect vision of the strabismic eye may either precede and be the cause of distortion, or may follow and be the consequence of it. In the former case the operation of dividing the affected muscle will not remove the imperfection of sight: in the latter it will.

"6. The morbid changes referred to in deductions 2 and 3 are competent to explain all the phenomena of the strabismus.

"7. The phenomena of strabismus cannot be answered for on any other hypothesis."

Such are the deductions that the author draws from the investigation of several hundred cases; and we consider that, in the main, his views are quite correct, especially in cases of confirmed squint; in all of which, no matter what may have been the primary exciting cause, the muscular apparatus is immediately affected, and most probably as stated in the general deductions which we have just quoted.

In the third chapter the author takes up the important question—the treatment of strabismus—and we so fully agree with him in regarding division of the affected muscle, or muscles, as *the only* means of cure, that we shall transcribe his words:—

"If the view," says he, "I have taken of the pathology of strabismus be correct, it must be obvious that no treatment can be of any avail, in confirmed squint, except division of the shortened muscle."

Again, he states that—

"The operation for the cure of strabismus is apparently so simple and easy of performance that nearly every surgeon thinks himself competent to undertake it,—and the result is, a large number of failures. This has, unfortunately, brought a very useful and beautiful operation into discredit, so that, even among my medical brethren, I have heard it stigmatized as uncertain. . . . . Now I am prepared to maintain that there are few, if any, operations so entirely unobjectionable and successful as the one in question, and, when properly performed, there is no case of strabismus that may not be improved, and but few that may not be completely cured by it."

Our own sentiments are quite in accordance with the preceding; and we can most truly say that, out of some hundred cases of convergent squint on which we have operated, it would be difficult to select any that had not been improved or cured by the operation. Mr. Holthouse advises subconjunctival section with a small knife; we prefer the scissors, and if the operator use a fine pair with blunt points, he need not have any difficulty generally in dividing the tendon, as it is held up by the hook, even through a very small opening in the conjunctiva. We do not think that every case admits of a like operation; for while in one case, where the tendon is of normal size, and where there are not any unusual attachments or adhesions, the operation can be easily performed through a very small aperture in the conjunctiva; yet in another, where the tendon is broad, and where the subconjunctival areolar tissue has become thickened and firmly adherent, we consider that a much freer opening will be required, and that the operation cannot be properly performed in such a case by simple subconjunctival section. This we have frequently found when operating; we have commenced by making a very small aperture, and then, by slipping in the hook and scissors, have endeavoured to divide the muscle, but our intention has been defeated by finding unexpected adhesions, and we have been obliged to enlarge the aperture, dissect down to the tendon, and draw it out, before

we could satisfactorily remove the existing deformity. The less the conjunctiva is cut and disturbed, the less probability is there of any unnatural eversion or protrusion of the eye occurring, or displacement of the caruncle; therefore, whenever the subconjunctival section can be properly accomplished, it is the best; but we repeat that our experience leads us to believe that in many cases it cannot be satisfactorily performed. We may also remark, that whenever we are now obliged to make a free division of the conjunctiva, we bring the cut edges together with fine points of suture, as pointed out by Mr. Walton; this step we have usually found effectual in preventing any one of the deformities to which we have alluded.

We must now conclude our review, necessarily brief, by recommending this Treatise on Squinting to the favourable notice of the profession. It contains much information on the subject, and proves the author to be an observant and painstaking practitioner. We should add, that the sections on "paralytic affections of the eye," and "certain forms of impaired vision," are well worthy attention: we have not time or space to give an analysis of them, but we call attention to them, as they contain original and sound views.

Sympathetic Nerve. By J. Drummond, M. D., Edinburgh. Article in the Cyclopædia of Anatomy and Physiology, Part xlvii. August, 1855.

On some of the more Obscure Forms of Nervous Affections, their Pathology and Treatment; with an introduction on the Physiology of Digestion and Assimilation, and the Generation and Distribution of Nerve Force, based upon Original Microscopical Observations. By Harry William Lobb, L. S. A., M. R. C. S. E. London: Churchill. 1858. 8vo, p. 312.

The Ganglionic Nervous System, its Structure, Functions, and Diseases. By James Geo. Davey, M. D., M. R. C. S. E. London: Churchill. 1858. 8vo, p. 309.

It cannot be much wondered at, that in the infancy of philosophy, the contemplation of living beings, their origin, growth, action, death, and decay, a series of phenomena so remarkable in themselves, and so utterly different from all others that might be made the subject of investigation, should suggest the idea of the presence of some supernatural power or agency of which the organism was merely the instrument. The prevalent belief in the inertness of matter, and that for the produc-

tion of motion or force in it some extraneous influence was required, rendered this idea more necessary, and contributed to its wider acceptance. The earliest form given to this agent seems to have been that of a sacred fire.

"The notion of the existence of an elementary fire—the soul of the world, the formative and conservative power of all nature," is said by Fletcher "to have descended from the Brahmins of India, through the renowned Hermes Trismegistus or Tot of glorious memory, and the Pastophori of ancient Egypt, to Thales and Pythagoras." It thus became the tenet under various names of the numerous schools which in later times flourished in Greece and Italy, some looking upon it, like the original propagators of the doctrine, as a kind of fire, others as a kind of air, ether, or spirit—the breath or spark of life, &c.—others as merely a kind of water. In the character of fire, the idea has been immortalized by the story of Prometheus, who is said by the poets to have vivified his clay statues by a fragment of it stolen from the chariot of the sun. By Galen it was divided into three families of spirits: vital, residing in the heart; natural, in the liver; and animal, in the brain. In this way, then, life came, according to Dr. Fletcher, to be regarded as a substantial existence like that coarser kind of life, which was one of the four reputed elements of all things; and as the latter, when it entered into bodies, was represented as the cause, as well of their general physical properties, as of the peculiar phenomena which they displayed during combustion—so the former, on becoming a denizen thereof, was described as the cause—first, of their organization; and afterwards, of the peculiar actions, by which, when so organized, they were distinguished.

This idea of life, as an entity distinct from the body, has come down to our own days, not, to be sure, as the Fire of the Egyptians, or the Spirits of Galen, or the Archæus of Van Helmont, but as the "Vital Principle," the name proposed by Barthez. It is difficult to arrive at any clear conception as to the nature of this principle. Hunter compared it to electricity and galvanism; by Lamarck it was considered a compound of electricity and light; and others consider it a principle "sui generis," and, with Alison, deny that it can be defined; but it seems to be agreed that it must have a fixed place and habitation. By Harvey it was located in the blood, or rather was the blood; according to others, a numerous band, it has its seat in the nervous system, especially in the cerebro-spinal portions of it; but on this point the neurologists are divided, many—of whom Fletcher may be looked on as the exponent—asserting it

is seated in the ganglionic department alone; and Dr. Davey, as we shall see, not only locating it exclusively in the ganglionic nerves, but even asserting that these hold a precisely similar relation to the otherwise insensible and inert frame, to the otherwise dull and unmoving organism, as the "Vital Fire" to the animated statue of Prometheus.

"Müller, it is well known," he says, "has written much about an '*organizing principle*,' a '*creative force*.' Admitting, for argument's sake, the existence of such a quality, it would seem to be first directed towards the development of a central organ or organs (the solar ganglion [*sic*], or the ganglia of the sympathetic), predestined not only to give *life* but *form* to the whole animal organism, and which, moreover, it creates; predestined to give to the 'organic creative powers,' as they are termed, their peculiar force and direction, which together determine the essential parts of the future animal, and its rank and position in the scale of infinite being; assign to the same its genus, or species, or variety, and give it, in short, a local habitation and a name—

> "'Human, angel, man—
> Beast, bird, fish, or insect—what no eye can see,
> No glass can reach, from infinite to thee;
> From thee to nothing.'—*Pope.*

Predestined, lastly, to hold a precisely similar relation to the otherwise insensible and inert frame, to the otherwise dull and unmoving organism, as the 'Vital Fire' to the animated statue of Prometheus."

It must strike all students of Dr. Fletcher's "Rudiments of Physiology" as a remarkable circumstance, that one who had written so forcibly on the nature of life, showing it to be essentially connected with the organism as the result of its organization, proving that vital "actions must be attributed, not to any one self-sufficient substance, however active, and however intelligent, but to countless irrational and unconscious forces, incessantly at work in every point of the system, in blind but implicit obedience to laws imposed on them by the Supreme Being, and adapted everywhere to the end to be fulfilled," should, in a succeeding chapter, argue that "irritability," meaning thereby, vitality, or the susceptibility to stimuli, whence the vital actions proceed, is possessed exclusively by the ganglionic system of nerves. It must ever be regarded as a noteworthy proof of the difficulty with which the doctrine, now so generally entertained, has been recognised and established,—a doctrine which teaches that each elemental part of the bodies of living beings is endowed with vitality, or irritability, or the

power of responding in a specific manner to the application of certain stimuli,—a power inherent in the tissue itself, and depending on its organization, just as any chemical compound, sulphuric acid for instance, depends on its composition for the peculiar properties it presents. In short, that muscle contracts when stimulated, nerve fibres convey impressions, renal epithelia secrete urea, and hepatic epithelia bile, when acted on by appropriate stimuli, for the same reason that a certain compound of nitrogen and hydrogen will neutralize acids, and one of nitrogen and oxygen will have a corresponding effect on alkalies. It will be observed that this doctrine applies only to the operations of the body, which are essentially material in their workings, objects, and effects. Those of the mind are of a higher order: they are not embraced in this discussion; nor is the immortality of man: a matter resting on higher grounds than any physiological doctrine, and not to be discussed here, —though now alluded to in order to prevent misapprehension, and that a protest may be made against the subject being brought into the argument.

The importance of this doctrine is every day becoming more evident; and though brilliant discoveries as to the functions of the cerebro-spinal system diverted attention from it for a period, all our best physiologists are now recurring to it, and, by using it as a guiding principle, are making important advances in physiology, and perhaps still more important in pathology. By its aid we can best understand that disease is often but the healthy reaction of tissues and organs to the influence of abnormal stimuli; and it places in its true light the important deduction to be drawn from this fact. That it is at the foundation of humoral pathology in its present form, is also evident.

It is interesting to observe how the theory of a vital principle as a distinct entity has been gradually receding before the advances of science. Philosophers have proved that matter is not inert: thus the keystone of the structure is gone. In like manner, an increased knowledge of physiology has shown that vitality is not the peculiar attribute of any one of the tissues—that the blood is not the life of an animal, or its exclusive seat. The cerebro-spinal system, too, has at length, even aided by galvanism and electricity, had to give up its claims, and now the doctrine has taken its last refuge in the sympathetic, or so-called ganglionic system. This, no doubt, results from our ignorance of this portion of our structure; for in proportion to our ignorance of the true functions of a part, is our liability to ascribe to it something wonderful. Consi-

derable advances have, however, been made during the last few years in the anatomy and physiology of the sympathetic nerves, of which a good account is given in the article by Dr. Drummond, to which we shall first direct our attention.

I. *Vesicular and Fibrous Neurine.*—It will probably now be universally admitted, except by Dr. Davey, that of the two forms of neurine, of which the cerebro-spinal as well as the sympathetic nervous systems consist, the vesicular is the dynamic portion, and that the use of the fibrous is confined to the conveying of impressions to, and influence from, the other. Some interesting observations have been made as to the connections of these two portions. It is well known that in the brain and spinal cord the ganglion vesicles present certain processes, which Todd and Bowman hint are probably continuous with nerve tubes. Their continuity has been demonstrated by Kölliker in the ganglia of the Amphibia, and his observations have been confirmed by others, especially Wagner, from his examination of the ganglia of fish. Some of the corpuscles or vesicles appear to be destitute of any processes ; others have a single process, some two, passing off generally at either extremity, but occasionally at a short distance from each other at one side of the body. Stannius proposes to call these respectively " apolar," " unipolar," and " bipolar cells;" and Remak has described " multipolar cells." In regard to the bipolar cells, when the nerve tubes come off at either extremity as they generally do, while one of them runs towards the periphery, the other passes towards the nervous centres ; when both pass off from one side, they run, according to Bidder, in the direction of the periphery. The connexion with the fibre is established by the cell-wall becoming continuous with the tube; the contents of the cell and tube are also continuous. Most of the cells that are seen in Mammalia are apolar ; it is, however, maintained by Wagner that the poles have been broken off during the process of preparing them for examination; and it is stated that Schrœder Van der Kolk has seen the bipolar cells in the cervical ganglia of man. The statement of Wagner is supported by certain observations on the ganglia of fish :—

" In the spinal ganglia of the ray the cells are easily isolated from each other; whereas in the abdominal ganglia it is very difficult, owing to the amount of surrounding fibrous structure, to isolate them. Now in the former, only bipolar cells are seen; whereas in the latter, most of the cells, when isolated, appear to be unipolar and apolar, although it would appear from the observa-

tions of Wagner and others that they are all bipolar, like those in the spinal ganglia. In the higher animals, especially in the Mammalia, the ganglionic corpuscles are isolated from one another with as much difficulty as those in the abdominal ganglia in the skate; and hence the probability that many at least of the unipolar and apolar cells which are seen in them belong to the bipolar variety in reality. On the other hand, that apolar and unipolar ganglion corpuscles really exist, and that, too, in considerable numbers in the ganglia of the higher animals, and also in those of the Invertebrata, seems to be shown by numerous observations on the smaller ganglia, where no preparation is required, and where, consequently, the above source of fallacy cannot intervene."

It appears, then, that apolar and unipolar cells do exist in the ganglia, and, moreover, that nerve fibres originate in these bodies. Volkman and Bidder have measured the nerves passing to and leaving the ganglia, and have found that more fibres leave them than had entered; and Kölliker confirms their observation. Engel also describes a peripheral ganglion to which no nerve fibres passed, though a number of nerves left it. Even in regard to the bipolar ganglionic corpuscles, it does not appear to be at all certain that they are all merely organs developed on the course of a nerve fibre arising in the brain and spinal cord. On the contrary, it would appear that several of the cells belonging to this variety must be also regarded as giving origin to nerve fibres in the same way as the unipolar cells. Thus Bidder and Stannius have seen bipolar cells, the nerve tubes connected with which did not run in opposite directions, one towards the brain and spinal cord, the other towards the periphery, but both ran in the latter direction.

"That most of the bipolar cells are, however, as Robin maintains, organs developed on nerve fibres of cerebro-spinal origin, in their course towards the periphery, there is no reason to doubt, and moreover, that several of these may occur in the course of a single fibre, between its central and peripheral termination, is also shown by the observations of Stannius on the fish, and by Valentin on the frog. Wagner has also observed two ganglion corpuscles occurring in the course of a single nerve fibre, at short distances from one another."

It is believed by Robin and Bidder that the ganglion corpuscles are of two kinds:—large, connected with the nerves of animal life; and small, connected with those of organic life. Their views have not been confirmed, and are opposed by Kölliker, Valentin, and Wagner. On the whole, it is probable there is some difference in the size of the two, though not sufficient to serve as a distinguishing mark, as proposed by Robin. Neither can we accept Wagner's statement, that the fibres of

the posterior roots of the spinal nerves are all connected with ganglion corpuscles, whereby they might be distinguished from motor fibres, for, according to Kölliker, in the higher animals not one of the fibres in the posterior root enters the ganglion at all; and Dr. Drummond's own examinations enable him to say that a great portion at least of the fibres pass the ganglion without forming any connexion with the corpuscles. Moreover, it is established that the multipolar cells are connected with motor fibres as well as sensitive.

With respect to the fibrous or conducting portion of the nervous system, there have been facts of equal or even greater interest brought to light. It is commonly taught that nerve fibres are of two kinds: tubular or white, the nerves of animal life; and gelatinous or solid, or gray, or the nerves of Remak, believed to be the nerves of organic or vegetative life. It is these latter that give the sympathetic nerves their peculiar colour, and not the presence of ganglion corpuscles scattered through them, as was supposed by Valentin, these being confined to certain limited parts. But though the fibres of Remak give the sympathetic nerve its peculiar colour, there seem to be good grounds for doubting whether they are nerve fibres, or merely areolar tissue, forming a sheath or protection for the proper fibres. It must be admitted to be unlikely, that there should be a set of fibres required, different in structure, for conveying impulses from the cerebro-spinal centres and from the sympathetic, however unlike the impulses might be, or the organs originating them. Valentin raised the question as to whether they really were nerves; he states that they do not arise from the ganglionic corpuscles themselves, but are continuous with a nucleated substance, that forms a sheath or capsule for these bodies, and that they are thence prolonged on the nerve tubes, and are to be viewed as merely discharging the part of a protecting covering or envelope for the latter. In their microscopic characters they correspond, he says, very closely with certain forms of white fibrous tissue, and in the main cord of the sympathetic, and in certain branches where the fibrous tissue is scanty, they are almost entirely wanting. Volkman and Bidder adopt the same view. In Mammalia they say the fibrous tissue is abundant; and the fibres of Remak equally abound. In birds there is less of this tissue, and they are not so numerous; and in cold-blooded animals, where there is very little interposed areolar tissue the fibres of Remak either fail altogether, or exist only in very small numbers. Valentin also states that these fibres cease in the mesentery of the horse, one or two feet from the point where the nerves enter the intestine. Kölliker gives the

weight of his authority against the claims of these fibres to be considered nerves; but Todd and Bowman, no mean authorities, take the opposite side. The argument is summed up as follows, by Dr. Drummond:—

"The chief grounds, then, on which it is held that the fibres of Remak are to be regarded as enveloping structures, and not as true nerve fibres, are:—1. The anatomical differences between these and the true or tubular neve fibre. 2. Their resemblance to certain varieties of white fibrous tissue. 3. Their connexion with the sheath of the ganglionic corpuscles, and not with these bodies themselves 4. Their absence in the final distribution of the nerve. 5. The increase of the thickness of the nerves *leaving* a ganglion being due, not to an increased number of the fibres of Remak, but to fine tubular fibres.

"On the other hand it has been stated, that the anatomical difference between the tubular fibres and the fibres of Remak is not a sufficient ground for believing that the latter are destitute of the properties of nerve fibres. All the nerve tubes in the embryo, even after it is considerably advanced in development, present much the same character as these fibres; and even after birth nuclei may occasionally be found existing in them. Again, as noticed by Todd and Bowman, the nerve fibres in the olfactory nerve resemble the fibres of Remak in containing nuclei, and also in the want of a double contour, as well as in their soft homogeneous appearance. When a nerve is divided, and a portion of it removed, the structure by which its continuity is restored presents much the same appearance as the fibres of Remak, and this for some time after the part supplied by the nerves has, to a great extent, regained its functions; showing that impressions may travel along structures not differing from the fibres in question. In reply to the second objection it is stated, that the difference between the fibres of Remak and white fibrous tissue is such as to preclude the notion of the one being a mere variety of the other. In the third place, it is said, it is by no means determined that the fibres of Remak are connected merely with the capsules of the ganglionic corpuscles, and, supposing they were so, that these also may be possessed of the properties of nerve tissue."

The true nature of these fibres must, then, still remain an open question, and opinions will vary as to which side the balance of the evidence as yet attained tends. It has been asserted, however, that, even admitting the fibres of Remak not to be nerves, there are certain nerve fibres characteristic of the sympathetic. In 1842 Bidder and Volkman announced the existence of these. They described them as being about half the diameter of those belonging to the cerebro-spinal nerves; they are further distinguished by their paleness, the

absence under all circumstances of the double contour, and the small quantity of curd-like contents which they present, even when they are examined some time after death; and by their yellowish-gray colour when they are in bundles. The increased size of the nerves leaving the ganglia was by Volkman and Bidder said to depend on these fine fibres. Valentin believes these so-called fine fibres to be no more than the fibres of Remak; but Reichart and Kölliker confirm in a great measure the account of Bidder and Volkman. Kölliker agrees that there are fibres in the sympathetic that do not arise from the cerebro-spinal, and that all arising in the sympathetic ganglia belong to the finer variety. He denies, however, that the fine fibres are peculiar to the sympathetic, as he has found them in various parts of the cerebro-spinal system. The conclusion Dr. Drummond arrives at is as follows:—

"It would appear, then, as Kölliker maintains, that there is no absolute distinction between the fibres of the sympathetic and those belonging to the cerebro-spinal system; the difference is merely one of relation and degree; while the nerves of the latter system consist *chiefly* of broad tubular fibres, the sympathetic is chiefly composed of fine ones."

Connexion between the Sympathetic and Cerebro-spinal Nerves.—It has always been a matter of great interest to ascertain the mode in which the sympathetic and cerebro-spinal systems are connected, and there have been many conflicting opinions on the subject. Recent researches lead to the belief that of the two communicating branches commonly found between each spinal nerve and the sympathetic in the higher animals, one being white and the other gray, and resembling the sympathetic in its aspect, the white consists of tubular fibres, coarse, fine, and intermediate in size, mingled in the same proportions as in the posterior roots of the spinal nerves; and the gray, of gelatinous and fine tubular fibres in the proportion of the branches of the sympathetic. The gray branch seems to be an offset from the sympathetic to the cerebro-spinal nerves, and it sometimes gives off filaments to the vessels in its neighbourhood, and presents ganglionic corpuscles in its fibres. As regards the white portion, there can be no doubt that all the broad tubular fibres are transmitted from the cerebro-spinal nerves, but the fine fibres may belong to either system; on tracing this white portion to the spinal nerve, the fibres are found to apply themselves in a direction more or less central, and on treating the spinal nerve itself

with a dilute solution of soda, most of these fibres can be traced into the spinal cord. In the opposite direction they can be traced beyond the ganglion, to the trunk of the sympathetic, and, according to Kölliker, to the peripherical branches, and in small animals even through the ganglia occurring in these latter. In the higher animals, at least, the fibres passing from the spinal cord to the sympathetic far exceed in number those passing in the opposite direction. Volkman has stated that the connexion of the two systems is through the posterior roots of the spinal nerves only; but Wutzer and Müller have shown that it is by both roots, and it seems probable that the cerebro-spinal fibres are derived from both.

Peripheral Distribution.—In the peripherical distribution of the fibres it appears that the branches passing to the viscera consist chiefly of the tubular fibres—those to the arteries of the fine and gelatinous fibres. The branches to the glandular organs are composed chiefly of the fine and gelatinous fibres, but a few broad fibres are also met with. They run in company with the blood-vessels and ducts, no nerves having yet been met with, running separately from those in the parenchyma.

Physiology.—The views of Bichat as to the sympathetic being entirely independent of the cerebro-spinal system were generally adopted until a comparatively recent time, when they were ably combated by Valentin, who endeavoured to establish the doctrine, commonly held before the time of Bichat, that the sympathetic and cerebro-spinal do not constitute two distinct and independent systems, but that the former is dependent on the latter for all its properties, and is in this respect to be regarded as one of the cerebro-spinal nerves. By Remak, R. Hall, and others, it has been suggested that the tubular fibres are for sensation and motion, and that the gelatinous preside over nutrition.

Volkman supports the view of Bichat: he regards the ganglia on the posterior roots of the spinal nerves as part of the sympathetic system, and all the fine fibres, he says, belong to it. The ganglia he considers to be nervous centres, meaning, Dr. Drummond says, by the term centre "an organ which serves as a regulating apparatus, and by which several separate and simple acts are combined into a single complex organic act." His argument may be briefly stated thus: muscular action implies the activity of motor nerves; it continues in certain muscles after the removal of the brain and spinal cord; digestion and secretion also continue. If these depend on certain nervous organs, and if the brain and spinal cord are not necessary, the

only centres they can depend on are the sympathetic ganglia. That they do depend on the nervous system is proved by the difference between their movements and those produced by physical stimuli.

It is obvious that such an argument only proves that the ganglia may exercise a regulating power. Tone is another function ascribed by Volkman to this system, but the experiments of Reid, Bowman, and others, sufficiently prove that this, as well as muscular action and secretion, are essentially independent of nervous influence, though they may be all controlled and modified by it. The conclusions arrived at by Dr. Drummond from the whole discussion are such as all, except Dr. Davey, will probably concur in. The presence of cerebro-spinal fibres in the sympathetic he holds to indicate the action of these centres; while the presence of gray vesicular neurine and of fibres specially connected therewith intimate the power of originating nervous force. It remains, of course, to inquire what are the nature and use of the force generated in these parts. In a standard work, such as the Cyclopædia of Anatomy, it was a wise discretion perhaps, to refrain from speculation, but Dr. Drummond might, we think, have gone a little further than he has done.

That *the sensory* properties of the sympathetic depend on the cerebro-spinal system, is too evident to require that it should be dwelt on here. There are some interesting points in connexion with these properties, however, that we would gladly discuss, if our space admitted, especially as to the absence of sensation during health in parts supplied by the sympathetic, and its presence in disease. It appears from the experiments of Valentin and Brachet, that division of the sympathetic branches does not cause pain; nor does irritation of them, unless it has been continued for a certain time; the ganglia and the main cord of the nerve, too, require to be irritated for some time before there is any manifestation of pain. The communicating branches to the spinal nerves are, however, as highly sensitive as any spinal nerve. It is well known that pain in parts supplied by sympathetic nerves cannot have its exact seat localized, as in parts supplied by the cerebro-spinal system direct. May not the explanation of all this be, that it is of the state of the ganglia that the sensorium takes cognisance, and not of the state of the parts supplied by their branches. The explanation, then, would be this:—Irritation applied to the extremities of the afferent sympathetic nerves is conveyed to the ganglia, and under ordinary circumstances excites a change there, and an impulse is reflected by the efferent nerves in the

regular course; but when this irritation is long continued, or violent, it causes a change, or sets up a morbid process in the ganglia, which is taken cognisance of by the sensorium by means of the sensory fibres in the communicating branches, and a sensation results. Morbid processes produced by remote irritations are not uncommon in the cerebro-spinal centres. It is thus that the irritation of worms or teething will give rise to convulsions, and it is probably by irritation conveyed from a wound, and causing increased polarity of the spinal cord, that tetanus is produced. The pain felt in the knee, and sometimes in the foot, in case of hip-joint disease, is another illustration of the same mode of action. The long-continued irritation in the hip is communicated to the spinal cord, where, in consequence, a morbid process is set up, and this is taken cognisance of by the sensorium, and referred to the parts supplied by the nerves that are connected with the irritated parts of the cord. This explanation follows naturally from the facts established by Brachet, which, indeed, Müller throws a shadow of doubt over; but as his statements are fully confirmed by the experiments of Valentin, whose results we have stated, we can no longer hesitate to receive them.

Volkman has proposed a somewhat similar explanation to that which has been suggested above; he ascribes the sensations that arise from prolonged irritation to the altered condition of the ganglionic fibres, but this is not so conformable to the analogy afforded by the cerebro-spinal system, nor would it account so well for the diffused nature of the sensations.

The *motor properties* are next considered, more especially as they are manifested in the heart, intestinal canal, genito-urinary organs, and pupil. It is very necessary, in considering these properties, to keep constantly in view, the fact that contractility is an inherent property of muscular fibre, and that, though the nervous system may supply the stimulus to call it into action, it does not supply the power of action.

It is sufficiently established by the experiments of Müller, Valentin, and others, that parts supplied by sympathetic nerves may be caused to contract by stimulating their nerves and ganglia. Dr. Drummond scarcely gives due prominence to the fact established by Valentin, that the same parts may be thrown into similar action by applying stimuli to appropriate parts of the cerebro-spinal system; Volkman, it is true, considers this as a reflex action.

"The effects produced," says Dr. Drummond, "upon the heart's action by stimuli applied to the central masses of the nervous system, and upon which the view that its movements depend on these

parts is chiefly founded, are explained by Volkman as taking place by reflex action through the medium of the sympathetic ganglia. The fibres which pass from the spinal cord to the ganglia stand to the proper sympathetic fibres arising in these, in the same relation in which the ordinary sensory fibres stand to the motor fibres of animal life."

It will be remembered that Volkman holds that all the fibres passing from the spinal cord to the ganglia are connected with the posterior roots of the spinal nerves alone; it has, however, been shown that this is erroneous, and that they are connected with both roots; and coupling this fact with the universality of the results obtained by Valentin, it must be admitted that a great part, at least, of the motor powers of the sympathetic are derived from the spinal cord.

It remains, then, to consider what are the special motor powers of the sympathetic. These we believe to be regulating and combining powers, which give the peristaltic and rhythmic character to the parts supplied by this nerve, such as the heart, intestinal canal, uterus, &c. The difference in the actions excited by physical stimuli in the voluntary and involuntary muscles is well marked. In the voluntary they are short, rapid, partial, and evanescent. In the involuntary they are slow, long, and assume the rhythmic character. An observation of Henlè's shows that this depends on the sympathetic ganglia. It was found by Bidder that in frogs which he fed with worms, and then removed the brain and spinal cord, the stomach, after a time had elapsed, was empty, part of its contents having probably been absorbed, and part passed along the intestines.

"The continuance of the movements of the intestinal canal after the brain and spinal cord have been removed, would seem to indicate that these are not the immediate centres on which their movements depend. The contractions which take place may be explained as due to the inherent irritability of the muscular fibres, while their type may be said to be owing to a peculiar arrangement of these, by which the contraction of one bundle acts as a stimulus to the neighbouring bundles, exciting these to contraction also; and in this way giving rise to the vermicular movements of the gut. It seems probable, however, that they are regulated by the ganglia of the sympathetic, especially since it has been observed by Henlè that in pieces of the intestine that have been cut away close to the line of attachment of the mesentery, the contractions produced by the application of local stimuli extend but a little way on either side of the point irritated, and are comparatively feeble. When a part of the mesentery is removed along with the portion of the intestine, they are more

powerful and more extended, and are most so when the intestine and mesentery are left in their normal relations."

The influence of the ganglia in producing the rhythm of the heart is perhaps still more evident. Valentin, Schiff, and Volkman all take this view. It may be shown that neither the peculiar arrangment of the muscular fibres, nor the mode in which the stimulus of the blood is applied to the heart, can account for its rhythm.

"To explain the rhythmic order in which these contractions take place, it is necessary to suppose that they, like movements of a similar kind—such as those of the respiratory muscles—are regulated by a nervous centre. The fact that the heart's movements continue after it has been removed from the body indicates, moreover, that the centre on which its movements depend must be contained in the organ itself. It has already been mentioned that in different parts of the heart are found small ganglia. These are believed by Volkman to be the centres on which its movements depend. These, according to him, act as organs from which the impulse to contraction proceeds; they are also connected with one another so as to act in concert, the impulses proceeding in such directions as to give rise to the regular succession in which the contractions of the different parts take place."

This is further illustrated by certain experiments—

"Especially by the observation first made by Henry, and afterwards by Müller, that solutions of opium, or of other narcotic substances, when applied to the outer surface of the heart, do not produce any obvious alteration in its action, whereas, when introduced into its cavities, so as to be brought into contact with its inner surface, their almost immediate effect is to cause this to cease. Again, when a stimulus is applied to one of the ventricles of a heart which has just ceased pulsating, the contraction thereby produced does not commence at the point irritated, as might be expected were the irritability of the muscular fibre alone concerned, but in the auricles, and is followed by contraction of the ventricles. Sometimes, indeed, stimulus applied to the ventricles is followed by contraction of the auricles alone. Even when the stimulus is applied to the apex of the organ, the contraction still commences in the auricles, and sometimes limits itself to these. The regular order in which its movements take place, so different from those produced in the ordinary muscles by the direct application of external stimuli, would imply that the impulse by which they are produced are conveyed in a certain definite direction to the different muscular parts of which the heart is composed; and this can only be supposed to be effected through the medium of its nerves."

This is still further indicated by the effects, as shown by

Volkman, which follow incisions made into the heart's substance:—

"When a transverse incision is made through the heart, between its auricles and ventricles, the former have been found to continue their contractions much longer than the latter; and if a longitudinal incision be made, gradually proceeding from the apex towards the base, the rhythm is preserved in both portions, until the heart has been divided half way; when the incision is continued further, however, the movements of either part become irregular. When the ventricle is divided transversely into two portions, that towards the apex either ceases its contractions immediately, or continues the same only for a short time, whereas that which is still in connexion with the auricles goes on contracting as before. It has been observed that in the heart of the frog there is one portion of the septum, between the auricles, which continues its contractions much longer than any other part, and in this portion the greatest number of the cardiac ganglia and nerves are situated."

The apparent peculiarities of the structure of the sympathetic nerve have always led anatomists to associate it with some peculiar office in the economy; and the facts which are being brought to light by the labours of modern observers tend greatly to simplify our views on this subject. The hasty sketch now laid before our readers, from the able article of Dr. Drummond, will probably lead them to agree with us in regarding the sympathetic as resembling closely the series of ganglia found in connexion with the suckers of the star-fish, rather than as being associated in any peculiar manner with the life of animals. In the star-fish, the ganglia in connexion with the suckers seem to be of local importance, and intended for the purpose of throwing the muscles moving the suckers into play on the application of any local stimulus. And so in the higher animals, the sympathetic ganglia are of local importance, regulating the movements of certain parts whose actions require to be specially combined. The discovery of a separate system of these nerves in the mesentery of birds, which has no connexion with either the cerebro-spinal or main sympathetic systems, confirms this view[a].

We pass over the interesting account given by Dr. Drummond of the influence exercised by the sympathetic on the movements of the iris and the state of the pupil—merely drawing attention to the fact, not mentioned by Dr. Drum-

[a] Dr. Drummond has not noticed this cord in birds; it is mentioned by Allen Thompson, but he does not say who discovered it.

mond, that tumours in the neck may, either by pressure or otherwise, so affect the cervical ganglia as to cause dilatation of the pupil. Dr. Banks, of this city, has published cases of aneurism in which this occurred; and we have ourselves had under notice a case of deep-seated abscess, in which the same symptom caused not a little alarm.

We may also remark, that MM. Budge and Waller are stated by Claude Bernard to have proved experimentally, in 1851, that the influence exercised by the sympathetic on the state of the pupil is derived from a region of the spinal cord comprised between the last cervical and sixth dorsal vertebræ inclusive, to which they gave the name of the cilio-spinal region.

The influence of the sympathetic on the vegetative processes is next considered by Dr. Drummond:—

"According to some," he says, "these processes go on independently of any influence exercised by the nervous system, while others maintain that the two are more or less intimately connected. Of the latter some believe that the sympathetic is the only part of the nervous system by which such influence is exercised; while others hold that it exercises no influence in this respect, which is not also exercised by the cerebro-spinal system."

Dr. Drummond advocates the view that they are essentially independent of the nervous system, but, at the same time, more or less influenced by it. Their independence is evidenced, he says, by the fact that they go on in the vegetables where there is no nervous system, and in the embryos of all animals before the nervous system has appeared. Both of these propositions are, however, disputed, as we shall see, by Dr. Davey, who holds that vegetables have a nervous system, and that in the embryo the ganglionic nerves are the first parts formed. Carpenter has very happily illustrated the connexion between the nervous system and these processes, by the instance of a man on horseback. The rider may urge on his horse to swiftness, or guide him in his course, or check him by the rein, but the power by which the horse moves is his own, and in no way supplied by the rider, or dependent on him. In the same way the power by which vital actions are produced is inherent in the tissues, but is subject to the combining and regulating powers of the nervous system. Unfortunately, these powers are ill defined, frequently misunderstood, and over-estimated. Even Dr. Drummond seems to have fallen into this error in some measure, especially, we think, in regarding the sloughing some-

times observed in typhus and other diseases, attended with great depression of the functions of the nervous system, as indicating the connexion between the nutritive processes and the nervous system. A more correct pathology would, we believe, refer the depression of the nervous functions and the sloughing to the same cause, the blood poisoning.

This controlling power of the nervous system is evident in the effects produced by certain emotions and passions on muscular movements, on secretion, circulation, and in a less degree on nutrition: the flow or absence of tears, of saliva, of milk, &c., and even of gastric juice under certain circumstances; are familiar examples: also the palpitation of the heart, and the blushing or pallor of the face. It is less evident as affecting nutrition: here the principal proofs of its operation are drawn from experiments, and some morbid states, many of which, as in the case of typhus above mentioned, seem to be capable of a different explanation. That the nervous system affects these several functions indirectly, by its influence in regulating the size of the small arteries, is proved by many facts. The muscularity of the small arteries is now well established; the sympathetic nerves maintain a close relation to these vessels, "clinging to them like ivy to a tree;" division of these nerves, or removal of their ganglia, will cause injection of the capillaries, and an elevation of the temperature, as well as an increased flow of the secretion from the glands in the region operated on, as of the tears, or of mucus. These effects seem to ensue immediately, and to be very persistent. In Reid's experiment the conjunctiva became injected "in a few moments" after dividing the sympathetic cord in the neck. In Valentin's the increased secretions "remained so, even after the lapse of several months." In Bernard's "the excess of temperature and sensibility was very intense a year and a half after the extirpation of the ganglion, when the animal was sacrificed for other purposes." But the vascular turgescence diminishes rapidly, though the increased heat continues. In Reid's experiment suppuration of the conjunctiva and inflammation of the cornea ensued, but Bernard[a] found that when the animal was otherwise healthy, this did not occur. When the cut extremity of the nerve was galvanized in the neck, these effects all disappeared; the vascular turgescence of the conjunctiva and ear, the increased temperature and increased secretions, as well as the contraction of the pupil before

[a] Monthly Journal.

mentioned, all disappear, but return on stopping the galvanism.

Walter and Brown-Séquard attribute the effects of the operation to paralysis of the arteries, but Bernard believes the phenomena are active; that "they are of the same nature as the vascular turgescence which arises in a secreting organ when it passes from a state of repose to an active discharge of function, and resemble the afflux of blood, and increased sensibility around a recent wound, or foreign bodies in the living textures —phenomena which are not due to mere paralysis of the arteries." Many of these latter phenomena are, we believe, due to the attractions and repulsions of nutrition, described by Alison as existing between the blood and the tissues; but the cessation of the former phenomena on the application of galvanism, and the fact that Valentin has seen the blood-vessels in a horse contract on stimulating the sympathetic, incline us to agree with the view that attributes them to paralysis, and consequent dilatation of the arterial coats.

The sympathetic is not, however, the only nerve that is concerned in these processes. The experiments of Schiff and Magendie show that the cerebro-spinal centres and nerves exercise an important influence over them. This is confirmed by the fact, that many of the glands are supplied chiefly, if not exclusively, by the cerebro-spinal nerves—as the mammary, salivary, and lachrymal. It is, moreover, interesting to observe that these are the glands which in the most remarkable degree manifest the effect of the emotions; the mammary glands particularly are much affected by them, and they have no apparent connexion with the sympathetic. We may also call to mind how much the so-called sympathies are manifested by these organs, as with the uterus, both in health and disease. The other parts, in which the emotions have most influence, as the heart and stomach, are also largely supplied by the cerebro-spinal system. This is what might be expected from the insight we have obtained into the workings of the emotions, and their connexion with the ganglia at the base of the brain.

We have thus followed Dr. Drummond throughout his article, one which, we believe, is not exceeded in interest or importance by any in the Cyclopædia of Anatomy. We hope that the necessarily imperfect sketch we have given, of the facts he has so well collected, will induce our readers to study the article for themselves. Before leaving Dr. Drummond, we may remark that there is one class of facts he has not alluded to, and which, if well authenticated, would show that

the nervous system exercises some further influence on the secretions, than through the alterations in the size of the blood-vessels. These are the alterations in the nature of the secretions, said to arise from mental emotions, of which the milk affords, perhaps, the best examples. Cases are recorded of women having, immediately after a paroxysm of passion or fear, caused the death of their infants, by applying them to the breast. If it be established that the child's death arose from the milk having acquired poisonous properties in consequence of the mental emotion of the mother, it proves that there must be some influence exerted on the secreting cells themselves; and that through the medium, most probably, of the cerebro-spinal nerves. It is believed by Dr. Carpenter[a] that these cases, and the symptoms in cases of shock, indicate that nerve force is being constantly correlated into cell force, and that its alteration or withdrawal is the cause of the phenomena. The subject is one, however, requiring further investigation.

We must defer until our next Number the observations we have to make on the other works, the titles of which we have given at the head of this article.

On Malformations of the Human Heart; with Original Cases. By THOMAS B. PEACOCK, M. D., Assistant Physician to St. Thomas' Hospital, and to the Hospital for Diseases of the Chest, Victoria Park. London: Churchill, 1858. 8vo, pp. 143.

To the professional reader conversant with the periodical literature of British Medicine, it is a needless task to introduce the author as a laborious and accomplished investigator of the more obscure affections pertaining to the circulatory system; as his contributions on various occasions for the last ten or fifteen years sufficiently attest the zeal and industry exercised in the pursuit of a favourite, but at the same time legitimate, study. In the year 1854 he delivered a course of lectures in St. Thomas' Hospital, on Heart Malformation, and by observation and research, the subject since became amplified to such an extent that it assumed the character of an essay on the subject; and having carefully revised his previous lectures, and collated materials from every available source, he systematized and classi-

[a] On the Mutual Relations of Vital and Physical Forces; in the Philosophical Transactions for 1850.

fied the several cases of malformation, reducing a most complicated and chaotic mass of records to simple pathological order. In the preface the subject is thus divided:—

"The subjects treated of in the following essay embrace:—

I. Congenital Misplacements of the Heart.
II. Deficiency of the Pericardium.
III. Malformations of the Heart, including,
  1st. Malformations dependent on arrest of development at an early period of fœtal life.
  2ndly. Malformations preventing the changes which should ensue after birth; and,
  3rdly. Malformations which do not interfere with the functions of the heart, but lay the foundations of disease in after life.
IV. Malformations consisting in the irregular development of the primary vessels.
V. Mode of formation; Symptoms and Effects; Diagnosis and Medical Management of cases of malformation."

The author gives an abstract of each case, the symptoms (where such existed) during life, and the post-mortem appearances, with a full reference to the source from which the information has been derived; and although some cases with which we are familiar have been omitted, still the number of quotations throughout the work is truly an example of industry and diligence almost beyond our comprehension, especially where time may be supposed to be fully occupied by practice. It is impossible, from the very nature of the work, to enter into any exact analysis of its contents, or endeavour to convey an idea of its intrinsic merits by a mere abstract of the cases. We must therefore be content with recommending Dr. Peacock's volume as a valuable *resumé* of all that is authentic in connexion with cardiac anomalies, which will prove both to the physiologist, as well as the practical physician, a work of easy reference, instead of the varied and extensive sources to which they were compelled to resort, prior to the publication of this useful treatise.

In conclusion, we cannot sufficiently express our admiration of the execution of the several plates illustrating many of the more interesting abnormities.

PART III.

MEDICAL MISCELLANY.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY[a].

(*Continued from* p. 239.)

SESSION 1857–58.

JANUARY 13, 1858.

THE PRESIDENT, DR. R. CORBETT, in the Chair.

Case of Popliteal Aneurism. By DR. S. HENRY HOBART, Medical Officer, Cork Dispensary.—"Anne Foley, aged 40, was admitted into the North Infirmary, under my father's care, August 12, 1855, with a tumour behind the knee. She stated that about five months previously she felt a very severe pain in the ham, which continued to give her great uneasiness; it was not until this had continued for a month that she first observed a hard lump, about the size of a walnut, behind the knee; she kept poultices to this for about two months, in hopes of 'bringing it to a head,' during all which time she could not rest day or night with the intensity of the pain. The tumour meanwhile rapidly increased in size, and the leg and foot became greatly swollen, the ankle being at one time more swollen than the knee. She now gave up the poulticing, thinking it did not agree with her. Soon after this she fell forward on the floor as she was trying to hobble through the room; and afterwards confined herself to bed for fear of getting any more falls; this fall (as she says) strained the veins, particularly one running up the thigh, which became hard and black; this appears to have been the biceps, as there is a hardness along the course of that muscle at present. One night she noticed, rather suddenly, a remarkable sensation in the limb, as if the whole swelling was being drawn up from the ankle

[a] These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—ED.

into the knee; from this time the swelling about the foot rapidly disappeared, but the tumour behind the knee increased considerably for some days. Since then, however, she thinks the tumour has rather diminished in size, and it is not quite so painful as at first. 8 o'clock P. M. Complains of desperate pain all about the knee, worse than any she has experienced for a long time past. She was given an anodyne, and the part was directed to be stuped and poulticed.

"August 13th. Got some relief by the draught, but the poultice only aggravates the pain; says the tumour has increased since yesterday; draught to be repeated this evening.

"14th. She thinks the tumour is much increased since admission, though no change can be perceived by others.

"15th. The case was very closely examined to-day; the tumour is principally situated above the knee-joint on the posterior aspect of the femur; a process of it seems to extend below the knee deep into the upper part of the calf of the leg, and another upwards along the course of the biceps muscle nearly half way up the thigh; the principal mass can be well defined at its lower margin, which is round and prominent; above, its extent is not so clearly marked, and laterally it seems to embrace the entire of the circumference of the limb; on touching it lightly there is no sense of fluctuation, but on pressing heavily with the thumbs placed on opposite sides of the tumour, an idea is communicated as if of deep-seated fluid, tensely bound up; the surface is smooth, and slightly polished from the stretching of the skin over it; the circumference of the limb at the largest part of the tumour is $16\frac{1}{2}$ inches.

"16th. Tumour looks larger, but measures the same as yesterday; some relief from the pain has been experienced from light friction with oil; there seems to be a sensation something like pulsation, but it is so vague and unsteady that it appears to be caused by spasmodic twitches of the hamstring muscles.

"18th. An exploring needle was now introduced; no pus was found; blood alone of a dark colour coming away in the groove of the needle; the sensation observed on the 16th, so like pulsation, is rather more obvious to-day, but still is very obscure.

"19th. Pulsation rather more obvious, and occasionally appears for a few seconds to be synchronous with the heart's action; then it seems as if the pulsation had been in the fingers themselves, or as if the eye had been deceived; again to appear more real for a little time; and again to become obscure. On applying the stethoscope, no sound can be heard, but occasionally the instrument feels as if it were throbbing against the ear; patient is greatly relieved by the needle puncture of yesterday. Some oozing has occurred from the puncture, of dark bloody-looking matter.

"20th. Had a severe rigor last night; pain worse in the tumour; complains also of headach; pulse 160, and feeble; tongue foul. She still has a sort of tremor over her; the tumour is decidedly softer, and has more the feel of fluid than before. The most careful examination now fails to detect any trace of pulsation, but this might be

expected from the changed state of the case. On the one hand, the softer and more flaccid state of the tumour is less suited to cause the circulation in the tips of the fingers to deceive; while at the same time the rapid and feeble circulation is less likely to communicate true pulsation to the tumour. She was now given a mixture containing aromatic spirits of ammonia, half an ounce; carbonate of ammonia, two drachms; camphor mixture, twelve ounces: to take a tablespoonful every second hour.

"21st. Febrile condition much relieved; the discharge from the puncture had been stopped yesterday; has returned; it looks like decomposed blood much altered in appearance. A consultation was held to-day to consider the nature of the case; no pulsation could be detected in any part of the tumour, but one gentleman perceived a slight bruit at its inner side. She was immediately recognised as having been in the South Infirmary, when the tumour was much smaller, at which time no pulsation was observed in it, and it presented rather the appearance of an enlarged gland, or of some form of encysted tumour, than of anything else. One medical man, who had frequent opportunities of examining the case in its early stage, states that from his recollection of it at that time, he is quite satisfied that, 'whatever it is, it is not an aneurism.' A large trocar and canula was introduced, which brought away a pretty free discharge of very dark blood, but this did not come *per saltum*, nor as if from a sack containing fluid blood,—was rather a free oozing, such as might be poured out by a very vascular part.

"The question of the nature of the tumour and the treatment to be adopted was now considered; and the opinions were divided between aneurism and fungus hematodes. The evidence of all who had seen the case in its early stage was strong against aneurism; moreover, the woman herself was never annoyed by pulsation in the tumour; and the vague and uncertain nature of the pulsation—if such it could be called—occasionally appearing to exist, was by no means sufficient to warrant a positive diagnosis of that disease; and all the symptoms might easily be caused by a highly vascular growth, such as fungus hematodes. The pale, unhealthy countenance of the woman also made her appear a likely subject for malignant disease. On the whole, therefore, it was thought advisable to postpone any interference for a short time, and to watch for some symptom which might render the nature of the case more clear. On sounding the woman, however, as to her willingness to submit to such operative interference as might be deemed necessary, she showed the most obstinate determination to have nothing done; seemed quite enraged at the mere insinuation of such a thing, and was with great difficulty induced to remain in hospital, by assuring her that nothing would be done without her full consent.

"23rd. Feverish again; pulse 130; pain severe in tumour; the discharge from the puncture has ceased to flow; complains that what was done to her yesterday (i. e. the introduction of the trocar), 'has stopped the discharge and injured her greatly;' the tumour is softer

and flatter; fluctuation is now quite evident, but there is no pulsation. She is excessively irritable, always grumbling and complaining.

"24th. Slight discharge has again occurred from the needle puncture, which has now more unmistakably the appearance of blood; poulticing gives relief for about an hour after it is applied; fever rather less.

"25th. A copious discharge of the same dark bloody stuff has been coming from the needle puncture; the trocar wound is quite healed, nor did matter at any time escape from this after the canula was removed. The discharge now coming away is of a most offensive smell, not resembling that from a malignant sore, nor from diseased bone, but more like that of a decayed human kidney; this poured out, on altering the position and sponging the part, as freely as matter from an abscess; and a small, but very soft clot, formed in the face; nothing like pus can be detected in the discharge. The tumour is greatly reduced in size, and is quite soft; no pulsation can be felt.

"27th. Still feverish; pulse 120; pain in tumour very severe.

"30th. Much better; fever almost gone; discharge becoming purulent.

"31st. A good deal of pain; discharge copious, but is now composed of about equal parts of pus and the same dark bloody matter; smell not so offensive.

"September 2nd. Discharge more purulent, but very abundant.

"4th. Profuse arterial hemorrhage set in at $2\frac{1}{2}$ A.M., and before assistance could be got the patient was dead.

"*Sectio Cadaveris.*—On cutting through the integuments over the popliteal space, we found a large clot of very dark colour occupying the site of the tumour; this was about the size of a man's fist, and was quite soft, as if recently formed; some ill-conditioned pus also escaped, but on extending the incision upwards and downwards, a great quantity of a mixture of pus and blood escaped; this had burrowed downwards to the lower third of the leg, and upwards to about the middle of the thigh. On evacuating the clots and pus, a round tumour, about the size of a billiard-ball, was seen lying near the bone; this was tolerably firm at its upper part, where it was whitish, and seemed like partially organized fibrine, but the lower portion was soft and dark-coloured, being a sort of intermediate substance between the fibrinous appearance of the upper portion, and the soft clots that surrounded the tumour. The femoral artery was found to be perfectly healthy in the middle of the thigh; this was cut across, and the lower portion removed, together with the entire contents of the popliteal space. On now passing a probe down the artery, it suddenly stopped near the tumour, and on slitting down the vessel to the point of obstruction, a papilla-shaped piece of fibrine was seen protruding into the side of it next the tumour, into which a probe could be freely passed at either side of the papilla. The greater part of the sack of the aneurism, as it now

undeniably proved to be, was strong, and resisted the pressure of the probe; but at the lower part, where it was dark-coloured and soft on the surface, it offered no resistance, and the probe passed out readily through it. The artery could not be traced along the back of the tumour, but terminated in a little cul de sac at the point of communication. On slitting open the tumour a cavity was found large enough to contain a hen's egg; this was smooth on the surface, and its lining membrane was perfectly continuous with that of the artery; it was about half filled with a sort of coagulum, which was rather tough on the surface, but which seemed to be very soft, or to contain fluid in its centre; the upper part, near the communication with the artery, was much more dense than the lower part, and terminated in the papilla of fibrine already referred to, which projected into the artery."

Dr. Finn communicated the particulars of the following case, and exhibited the pathological specimen:—

Case of Abdominal Aneurism.—James Barrett, aged 30, car-driver, was admitted into the North Infirmary on the 7th December, 1857. History of case:—His habits have been intemperate till a very recent period, but his health had been uniformly good till about four years since, when, for the first time, he began to suffer from pain referred to the loins. This pain, which was comparatively slight at first, gradually increased in severity, and compelled him after the lapse of a year to apply for medical advice at the North Infirmary, into which he was received on the 17th October, 1854. At this date the existence of aneurism was first recognised. Having remained on this occasion forty-four days in hospital, he was dismissed, greatly relieved. He was readmitted on the 5th January, 1855, and remained in hospital forty-nine days. On leaving he immediately resumed his usual occupation; in this occupation he had been almost uninterruptedly engaged during the interval between March, 1855, and the date of his final readmission to hospital on the 7th December, 1857.

Present Symptoms.—Dull lumbar pains; pain of a more acute character referred to the left ilium; augmentation of pain in both situations towards night. A sense of soreness in the epigastrium and abdominal viscera; in the epigastrium was observed great intolerance of pressure; in the erect position he felt as if the back were opening. Countenance sallow, and expressive of anxiety; age apparently more advanced than that recorded. Abdominal walls unusually tense; and considerable turgescence of the superficial veins. Tongue clean, costive habit; urine in appearance natural, specific gravity 1017. Pulse in recumbent position 66, communicating a peculiar thrill to the finger. In the left hypochondrium was discovered a tumour of moderate size, communicating a slight impulse to the hand; but the intolerance of pressure already noticed precluded the possibility of tracing its outline. The heart's

sounds were normal; but its impulse was somewhat augmented in force. Auscultation revealed a loud bellows murmur in the epigastrium, which was distinctly audible for about three or four inches downwards to the left of the mesial line; the pitch of this murmur was increased by deep-seated pressure. In the erect position it was barely perceptible; but on the posterior part of the trunk it was inaudible in any position.

December 30. Pulse 108; passed a restless night; suffered from excruciating pains referred to the left side and hip; complained of nausea; pain most severe towards evening.

December 31st. Pain has increased in intensity since last report; more particularly in the left ilium.

January 1st, 1858. Pulse 84; pain has in some measure abated since last evening, having yielded to anodyne draughts composed of Battley's sedative and chloric ether, a combination nearly similar to that employed by Dr. Law with so much benefit in such cases.

January 4th. Pulse 102, feeble; complained of pain in the cardiac region and left hypochondrium. This pain supervened quite suddenly, and was referred on the occasion of the clinical visit to extravasation of blood from the sac into the cavity of the pleura.

January 6th. Pulse 108, feeble; suffered since last visit from the most excruciating pain in the left chest, anteriorly and posteriorly; has had no sleep; cold perspiration for the entire night.

January 8th. Passed a tranquil night, the pain having been relieved by sedative draughts, repeated at short intervals; died suddenly this morning at $7\frac{1}{2}$ o'clock.

The autopsy took place about four hours after death. On raising the sternum some blood escaped from the left pleura. This cavity contained a very large quantity of blood, generally in a fluid state, with a few small coagula. The abdominal aorta was the seat of an aneurism of considerable size, which involved about four inches of the vessel, having commenced where it passed between the crura of the diaphragm. At the proximal side of the sac, above the cœliac axis, and on its posterior aspect was observed a rent of about half an inch in width, opening into the left pleura; through this rent the more fluid contents of the aneurism escaped. The distal end was filled with solid fibrinous coagula. The sac was united by the most rigid adhesions to the corresponding vertebræ. The bodies of four vertebræ (the last dorsal, and three lumbar) had undergone destructive ulceration to a considerable extent, and communicated a most painful sensation to the fingers when moved along the unequal surface thus produced. The thoracic and abdominal viscera presented no deviation from the normal state.

Remarks.—The experience of this case would appear to indicate that a very considerable duration of life is compatible with abdominal aneurism, a period of thirty-nine months (less nine days) having intervened between the first recognition of the disease and its fatal termination in January, 1858; and the lumbar pains com-

plained of in the autumn of 1853 render it highly probable that the aneurism dated its inception from this period. He had been altogether 125 days under observation.

Pain played a prominent part in the history of the case. The pain, which was of the intermittent kind, was characterized in the manner described by Dr. Law[a], by a nocturnal paroxysm of great severity; it was sometimes referred to the last dorsal and lumbar vertebræ; at others to the abdominal viscera; but more uniformly to the left ilium, in which latter situation it simulated hip-joint disease. The migratory character of the pain impaired, however, its diagnostic value, and no reliable conclusion could have been deduced from a *general sign*, presenting the succession of changes above noticed. A remarkable fact in connexion with this symptom was its suspension for an interval of nearly three years, during which he had been engaged in his ordinary employment. Supposing the pain to have resulted in the first instance from erosion of the vertebræ, consequent upon the aneurism, how reconcile the absence of this symptom for the long interval referred to, with the probably progressive character of the pathological changes which called it into existence?

The importance of the *physical signs* was exemplified in this instance, the existence of aneurism having been recognised at an early period. Was the influence which the erect position exercised in modifying the auscultatory phenomena to be ascribed solely to hydrostatic pressure? The altered relations of the surrounding viscera to the aneurismal sac, and the tension of the abdominal muscles, might have contributed, at least in part, to the result.

Professor O'Connor related several cases of *Gangrene of the Lung*, and concluded with the following remarks:—

"In addition to the cases which I have now detailed to the Society, it has been my lot to see, from time to time, a considerable number of instances of this affection, more than could possibly fall under the notice of any one physician, if he had not been engaged, as I was, for several years in conducting the largest workhouse hospital in Ireland, and that during a period of famine,—I will take the liberty of laying before you my experience under the heads of the cause, diagnosis, and curability of that disease.

"Under the first head I have to observe, that never, except in the case of Parrell, have I found pulmonary apoplexy precede the gangrene, though it ought to be expected to result from this cause, where the lung tissue is much injured, and the circulation in the part obstructed by the effused blood acting as a foreign body. Still, how frequently do we find extensive disorganization of the lung arising from this cause, without gangrene following. The fact that hemoptysis so frequently succeeds gangrene, may occasionally mislead in tracing the history of the disease. Tubercular disease in the

[a] The Dublin Journal, first Series, vol. xxii. p. 389.

lungs seldom gives rise to gangrene. This might be readily inferred from the seat of the latter disease being most frequently in the inferior part of the lung, where tubercle is seldom found in the earlier stage of its development. The only way in which it may arise from this cause is, where tubercular infiltration suddenly seizes on a large portion of a lung, not only interrupting the respiration in the diseased part, but also arresting the circulation. In such cases, after remaining some time in a solidified state, the affected portion of lung may run into rapid suppuration or gangrene. In this manner I have seen nearly the entire left lung of a child, eight years old, after remaining some days in a state of solidification resembling pneumonia, suddenly pass into gangrene, producing a fœtor so abominable that it was impossible for even the parents to remain long in the room in which the child was confined. That gangrene is rare, as a consequence of pneumonia, is admitted on all hands. Even where the lung runs into suppuration in such cases, the expectoration is rarely fœtid; and where such is the case, the fœtor is different from that of gangrene. The gangrenous eschar is, in most cases, very circumscribed at the commencement, and in this respect differs in character from pneumonia. A rusty-coloured sputa frequently accompanies the disease, which is the result of consecutive inflammation in the surrounding part of the lung. We are then led to the conclusion, that if gangrene results from inflammation of the lung, it is inflammation of a special kind,—such, it may be, as carbuncular inflammation in the areolar tissue of external parts. Its rapid spread through a lung previously healthy, when once established in any point, only corresponds with the character of gangrene in other parts of the body. The great prevalence of this disease, at the time when scorbutus was epidemic amongst the humbler classes during the years of famine, and for some time subsequently led me to the conclusion that the condition of the blood in this latter affection predisposed to, if it did not produce, gangrene of the lungs. The rapid destruction of the eyeballs in ophthalmia, and the frequent sloughing of the penis in gonorrhœa during the same period, was, in my mind, attributable to the same cause. We can readily understand that the alteration of the blood, peculiar to scorbutus, existed in many cases prior to the ordinary external manifestations by which it is generally recognised, in the same manner as we know that the syphilitic poison lurks in many constitutions for years, without ulcer or eruption to manifest its presence.

"The diagnosis rests partly on the fœtor from the breath and expectoration, and partly on the stethoscopic phenomena. Amongst other writers, Dr. Stokes especially refers to the fact that fœtor of the breath is frequently found in cases of chronic bronchitis, scarcely distinguishable from that of gangrene. Still, with the greatest respect for so high an authority, I cannot help thinking that the fœtor of gangrene is entirely *sui generis*, and scarcely possible to mistake for any other smell, however disgusting. A means of distinguishing them on other grounds is also presented in many cases

thus. The fœtor in a case of circumscribed gangrene (that alone in which any difficulty of diagnosis could exist) is sometimes absent for hours, till a fit of coughing brings up some of the offending matter; turning on the side, or moving in bed, may produce this fit of coughing. In some instances the patients stated that they found the fœtor on their palate before the coughing came on, which soon poisoned the whole atmosphere of the room. Nothing could exceed the violence of the paroxysm of coughing in these cases and in nearly all cases of gangrene, the vapour from the diseased part being a severe irritant to the mucous membrane of the larynx and trachea.

"In circumscribed gangrene I never could discover the signs of a cavity in the earlier stages, but in every instance found some one part of the lung over which there was dulness on percussion, and an absence of vesicular breathing not replaced by bronchophony or bronchial respiration, but by a few subcrepitant râles heard on forced inspiration, more like a sound caused by the forcible displacement of the parenchyma of a solidified lung than if produced in either bronchi or air vesicles. Most writers speak very despondingly of the curability of gangrene of the lungs, and some think such cases utterly hopeless. In this opinion I do not entirely share. Two of the most marked cases I had ever to treat recovered perfectly. One of them had since to undergo a very searching examination for the purpose of effecting an insurance on his life; and the other has been under my observation for a period of five years since his illness, and enjoys perfect health. A third case died of another affection more than twelve months after I had diagnosed the existence of gangrenous eschars in his right lung, and I had subsequently an opportunity of showing this lung to the Society, containing two cavities with walls perfectly organized, and evidently prepared to cicatrize if the patient had lived much longer."

January 27, 1858.

The President, Dr. R. Corbett, in the Chair.

Dr. S. Henry Hobart exhibited the brain of a man named Daniel Connor, who had died in the Lunatic Asylum a few days previously, during an epileptic seizure. The man was admitted to the Asylum on the 14th of July, 1857, having then come from the Fermoy Union Hospital; he had some time previously received an injury of the head, for which he entered that hospital; from the immediate effects of the injury he soon recovered, but he was seized at intervals with epileptic attacks, which were so frequent as to render him unfit to obtain a livelihood for himself; and as those attacks showed no disposition to cease, he was admitted into the Cork District Lunatic Asylum. Here the fits continued to be both severe and frequent; during the intervals, however, his mental and bodily powers were not materially impaired, and he was able to make himself useful in many ways in the Asylum.

On the 24th of January he was seized with an attack more violent than usual, in which he died. On examining the brain, twenty-six hours after death, it was found to be remarkably healthy in its structure generally; there was no congestion nor appearance of inflammation in any part; but the right posterior lobe was greatly atrophied, indeed it appeared as if about half the portion of brain corresponding to that lobe had been absorbed; this had principally taken place on its upper aspect and appeared to have been affected partly by dilatation of the posterior cornu of the lateral ventricle, and partly by absorption of the external surface of the brain; for the fossa in the skull, corresponding to this lobe, was in a great measure filled up, showing that this portion of the brain was not as prominent as the same part on the opposite side; while the lining membrane of the ventricle could be seen distended with fluid, the layer of cerebral matter separating it from the arachnoid being very thin, and over a small extent being altogether absent, so that the fluctuation of the fluid contained in the cornu of the ventricle could be very distinctly perceived on the surface. On cutting into the ventricle a large quantity of fluid escaped, and a cavity was exposed more than sufficient to contain a large walnut. The structure of the brain was found to be decidedly firmer than the average, and such as would be peculiarly well adapted for anatomical demonstration.

The President then brought the following case of *Hemorrhage from the Bowels* before the notice of the Society:—

"Mrs. ——, aged 26, of petite, fragile form, generally healthy, and of active habits, mother of four living children (having had three miscarriages), was confined in July last without medical assistance. Her labour was protracted, and the placenta, after the birth of the child, was retained for over six hours. Pending the retention there was a considerable hemorrhagic drain, but not such as to cause alarm to an old ignorant midwife. Her husband, however, feeling uneasy about her, summoned two medical men resident in the neighbourhood, but the lady refused to see either, having an objection to male interference. After some time the placenta was thrown off, followed by considerable hemorrhage, which caused much prostration. However, at the end of three or four weeks, the patient was able to go about, still feeling very weak, but would not have medical advice. She did not suckle her infant, and about the end of six weeks after recovery there again occurred considerable hemorrhage, which has to the present continued to increase every fourteen or fifteen days. On the 14th of last November she arrived in town, a journey of twelve miles, when I was called on to visit her. Having previously known her, I was struck with her exsanguine and leucophlegmatic appearance, and on inquiry I then found, in addition to the above history of her symptoms, that, for the fortnight preceding, she had been suffering from profuse intestinal hemorrhage; and on my visit the servant showed me a *pot de chambre* containing upwards of four-

teen ounces of blood, passed a few minutes before I arrived. This was unmixed with feculent matter. The servant said to me :—'Sir, I only wonder, if she were a cow, she could bear the loss of blood for the last fortnight.' There was no fulness of the abdomen, no tenderness on palpation or pressure, the only symptom complained of being a pain at the epigastrium, immediately preceding the action of the intestines, which pain subsided on the passing of the blood. The pulse steady at 72, but debile; tongue clean and moist; appetite capricious; occasional thirst; chilliness of surface; urine in good quantity, and normal. Pending this hemorrhage from the bowels, the uterine flow was arrested. I at once directed rest in the recumbent position, farinaceous diet, and commenced giving a mixture of acetate of lead and opium; of the former of which she took four scruples, and of the latter a drachm and a half in divided doses, within eighteen hours, but without benefit. I then changed to gallic acid, five grains every two hours (with an opium and alum pill at bedtime); of which she took within twenty-four hours 100 grains. Still the hemorrhage continued to the amount of some twenty ounces per diem, a quantity which the patient and her servant looked upon as trivial in comparison to former losses. Finding that on one or two occasions small portions of pulpy feculent matter passed down with the blood, I ordered a mixture of four ounces of infusion of roses, four drachms of sulphate of magnesia, and a scruple of alum: of which half was to be taken immediately, and the remainder after six hours if the bowels should not act. The whole had been taken, and at my evening visit I found a large dejection of greenish fæces, semi-solid, and altogether free from hemorrhage. This was on the fourth day of my attendance. The hemorrhage altogether ceased, and I sought to restore the tone of stomach and system generally; but, strange to say, within a week of the yielding of the intestinal bleeding, that from the uterus reappeared, and not of a catamenial character. However, it lasted only a few hours, and ceased.

"The patient was, fortunately, of an active, lively disposition, although of delicate frame; and, finding herself so much better, she determined on returning home, which she did about the 2nd of December. She would not permit any examination of the uterus, nor could I persuade her to use a lavement for the relief of the bowels; so that, although so far relieved, I should much fear there will be recurrences of hemorrhage; and, in truth, I feel at a loss to account for the source of hemorrhage from the bowels. She is at present taking sulphuric acid three times a day, and when I next hear from her I shall report progress to the Association.

"Calling to recollection the case of hematuria which on a former occasion I detailed to the Association, I was struck with the failure of the usual anti-hemorrhagic remedies administered, as well in it as the case now brought forward; and therefore it was that I decided on a trial of the alumino-magnesia draught, which in the present, as well as in the former, seemed to make the best impression on the hemorrhage. Whether to attribute the result in the one or

both to the 'post hoc, propter hoc,' I cannot take upon me to say. It may have been that the astringents previously administered had, by the time that the saline astringent aperient had been given, commenced their special action; but there can be little question that the favourable result was *after* the alumino-saline draught in both. Since the 2nd December I have seen the patient twice or thrice for a day or two each time, and on each occasion the uterine hemorrhage was the only cause of complaint, and as one of a chief means of at least moderating this flow, I could not persuade my patient to observe *perfect rest* in the recumbent position; so far from it, that the theatre was one source of her recreation. How she has so far stood up against the losses which she has suffered, is to me extraordinary. There may be, for all I know, a very sufficient cause for this uterine hemorrhagic tendency; but without a satisfactory examination it is impossible to account for it. I do not look upon the patient as having a hemorrhagic diathesis; nor, on inquiry, can I ascertain that there is foundation for forming such an opinion. I directed partially cold bathing, application of cold over the hypogastrium, and recommended vaginal injections of cold fluid, which latter would not be listened to. I also gave sesquichloride of iron, and regulated the state of the bowels; but, up to a fortnight ago, without making an impression, further than the patient's stating, that although the hemorrhage recurs, it is less abundant, and of shorter continuance. I have not hesitated to assure her, that unless the cause can be arrived at, and if possible remedied, I look upon her life as a precarious one; as these symptoms, so long existent, must necessarily terminate in a break-up of the constitution, probably producing dropsical effusion into some of the vital cavities. Within the last few days I have had a letter from this lady describing urgent dyspeptic symptoms, but not alluding to the hemorrhage.

"P. S.—The intestinal hemorrhage has not recurred, and the uterine has been arrested by pregnancy."

Case of Cancer of the Œsophagus. By Thomas Gregg, M.D., Surgeon to the South Infirmary.—"Denis Flynn, aged 60, was admitted as an intern patient into the South Infirmary, January 8, 1858, for supposed stricture of the œsophagus. I found it very difficult to get much information from him as to the previous history of his disease, but the following was obtained:—He has always been a man of very temperate habits; he never had received any injury to account for the disease. He first noticed, about twelve months prior to his admission into hospital, a slight burning sensation, as if the food, while passing through the œsophagus, was too hot; this ceased immediately after it had passed into the stomach; this sensation was so transient and trivial in its character that he did not apply for any advice; it continued without getting worse for about three months, when he first found that the food suddenly stopped about half way into the stomach, and required some fluid to get it down; this check was only occasional, and was generally

experienced at first during the commencement of the meal, for when the food passed into the stomach no further difficulty existed during the progress of the meal; however, it steadily increased until the difficulty of swallowing any solid food was very urgent. During this time he had been under the care of a medical practitioner in the country, who treated him, with occasional partial relief. About eight months after the commencement of the attack, an instrument was passed into the stomach, which he states always gave relief for some days, and was introduced without difficulty; this has not been attempted for the last fortnight prior to his admission into hospital. His present appearance is that of a man in the last stage of phthisis, very much emaciated; countenance pale, and expressive of much anxiety; tongue clean; fauces and pharynx very pale; the latter has some varicose superficial vessels ramifying on its posterior surface; he says he can swallow better sometimes than at others; his sleep is very much disturbed; pulse quick and weak; slight cough; breathing stridulous. On the application of the stethoscope no disease of lungs was discoverable; vesicular murmur audible over both; the stridulous sound was conveyed to the ear evidently from the trachea; there was some soft bronchial rale on the posterior surface at the root of the lungs; no bruit of any kind could be detected; the impulse of the heart was stronger than natural, but no valvular disease existed; he complained of pressure over the second to the fourth dorsal vertebræ giving him much pain. I now tried to pass a bougie through the œsophagus, but could not get a small one farther than the commencement of the œsophagus; failing, I did not attempt further, although he said a moderate-sized one had passed easily before his admission.

"A question here arose, what could account for the stridulous breathing? That it was caused by some pressure there was no doubt; and more than likely by some tumour in the thorax; none could be detected in the throat; and that it was not aneurismal, the stethoscope proved. The post-mortem, which will be detailed, fully shows the true cause. I must confess I did not suspect malignant disease of the œsophagus; except the occlusion, there was no symptom to indicate such an amount of disease as absolutely existed. The principal treatment consisted in antispasmodics, opium, attention to the alimentary canal, nourishing fluid diet. About a fortnight after his admission he complained of pain in the right side, which was removed by counter-irritation; and on the visit of the morning of the 18th there was considerable bronchitis over both lungs, and he died rather suddenly the same evening, after having taken some fluid, as if suffocated.

"*Post-mortem*, eighteen hours after death.—Body very much emaciated; rigor mortis very complete. Upon opening the cavity of the thorax, and removing the sternum and anterior portion of the ribs, the lungs rose out of the cavity, as if they were too large for the space; they were adherent at either side to the costal pleura by old standing thick deposits of lymph, and all the anterior

surfaces were thickly coated over with the same; a considerable quantity of serum occupied each pleural cavity, as well as the pericardium, and seemed to be the cause of the lungs being pushed so far forward. On the removal of the parietes, the posterior surface of both lungs was found much congested, and the right lung, particularly at the root, was of a scirrhous hardness; the heart and large vessels were quite healthy. Upon attempting to remove the œsophagus I found it firmly bound down to the bodies of the second, third, and fourth dorsal vertebræ, and it was with some difficulty I could remove it; the lining membrane of the bodies of these bones seemed to have partaken of the same disease as the œsophagus, the coats of which answering to this part (about four inches altogether) being completely disorganized. I have removed this for the inspection of the members of the Society; it will be perceived it is quite cancerous, and now we can account for the stridulous breathing; the trachea above its bifurcation is firmly adherent to the hardened and thickened coats of the œsophagus, and a considerable opening, by ulceration, has taken place between one tube and the other; I think this opening was the immediate cause of the sudden death of the man; by allowing a free communication with the lungs, the fluid he had attempted to swallow, more than likely, passed in, and blocked up the air-passages. I should have mentioned before, there was some fluid found on cutting into the lungs; the remainder of the œsophagus and stomach was quite healthy, and no further trace of cancerous deposit could be detected.

"I do not at present remember to have seen a similar case recorded where ulceration had taken place between the œsophagus and trachea. The great extent of disease shows us that we ought to be exceedingly cautious in the use of bougies; the contraction is so great that a common crow-quill will scarcely pass through it, and it is difficult to imagine how an ordinary-sized bougie could have been passed into his stomach six weeks prior to his death. As far as treatment is concerned in these cases, I fear very little can be done except to palliate; and fortunate it is for the profession and humanity that such cases are of rare occurrence.

"Are there any symptoms whereby we may be guided in forming an opinion as to whether there is cancerous degeneration of the œsophagus? There was nothing here to guide us; no pain; nor was there that peculiar appearance which usually indicates the cancerous diathesis. I do not know of any; for we may have had the same as this man complained of produced by other diseases; but I think that we ought to look upon all cases of occlusion of the œsophagus occurring without any assignable cause with a great deal of suspicion."

A few Remarks on Irritation of the Spinal Cord. By John Williams, A.B., M.D., Surgeon to the Cork Eye Dispensary.—"I am not aware of a subject more replete with interest, or better calculated to repay careful investigation, than that class of per-

verted functions, whether of circulation, nutrition, or reflex nervous action, which, when grouped, form a lucid exponent of irritation of the spinal cord, but, when isolated and considered separately, are calculated to divert the attention from the *real* seat of morbid action to the sympathetic functional derangement of whatever organ or organs may appear to be more particularly affected, thereby misleading the practitioner, and urging him to the adoption of remedies useless, perhaps hurtful. In such cases as those to which I have made allusion, I consider it of primary importance, and a potent help towards the forming an accurate diagnosis, that extreme care be taken in the grouping of the existing signs and symptoms; for in the generality of diseases or perverted functions, but more particularly in the subject of these remarks, how characteristic soever a single sign or symptom may be, it would be a dangerous error to regard it as pathognomonic without the concurrence of others. To illustrate these observations, I adduce a case in which the peripheral extremities of branches of the lumbar plexus were affected, and the existing local symptoms pointed directly to the spinal cord; but the absence of those general symptoms which, when associated with the former, would stamp the impress of truth on the diagnosis, proved incontestibly that the seat of the affection was not central, but peripheral.

"We must not, however, forget that irritation of the peripheral extremities of the spinal system of nerves, which, according to him to whom the entire medical community will for ever owe a debt of gratitude for his almost complete elucidation of nervous function, the knowledge of which previously had been enveloped in a thick mist—I allude to the late lamented Marshall Hall—is the true sympathetic, may be propagated backwards to the spinal centre itself. In the treatment of various affections of the cord we take advantage of the direct analogy which appears as if reflected from this fact, and we witness many examples where the normal function of the spinal cord was restored when the *local* treatment was directly applied to the peripheral extremities of its nervous branches. Amongst the variously perverted conditions in which we frequently observe the function of the cord, strictly independently of inflammatory or any other diseased action, we experience little difficulty in proving satisfactorily to ourselves that local irritation of the nervous extremities is, in many cases, the sole cause of these abnormal conditions; for example, convulsive action of the muscular system, the result of local irritation of the nervous extremities from pressure in the teething of infants; from the presence of worms in the intestinal tube, or of undigested matter in the stomach; or in more local spasmodic action, such as in many attacks of asthma from irritation in the lungs from odours, or subtile solid particles; or in the stomach and intestinal canal; the crowing inspiration of infants, not dependent on pressure on the pneumogastic nerve, but from local irritation; tenesmus and strangury, also afford like examples of disordered reflex action of the spinal cord, from local

irritation of the peripheral extremities of its nervous branches. In hydrophobia we have an example of perverted reflex action resulting from local *irritation*, if I may so speak, of a nerve of special sense, as evinced in the supervention of a paroxysm when fluid is either seen or heard. In those cases in which that group of morbid conditions, to which the term hysteria has been given, exists, mental emotions, and many other causes, give rise to perverted reflex action of the spinal cord; but, as in the case I purpose to bring before the Society this evening, a purely mental cause is *alone* sufficient to *irritate* and to stimulate to morbid action the cranio-spinal axis; and I do not hesitate to state it as my decided conviction that if the operation of that cause continue long in existence, the mere irritation of the cord would ere long degenerate into a more serious morbid affection, the approach of which is almost instantly heralded by its characteristic physical signs and symptoms.

"The most fertile cause—purely mental—of spinal irritation is mental depression. The ability of emotional causes to excite to morbid action, through the medium of the cerebrum alone, may be questioned, if we consider that mechanical irritation, such as an apoplectic clot, or direct pressure in experimentalizing on the inferior animals, applied to the cerebrum, fails in exciting the spinal cord to action; the convulsive actions which are present, in some cases where the cerebrum alone appears to be affected, are due, according to Marshall Hall, to the implication of one of the membranes of the brain in the disease, for branches of the spinal system being distributed to them, the cord through them becomes irritated. Still, in epilepsy and other diseases of a convulsive nature, the cerebrum, too, is more or less affected; this is particularly seen in those morbid conditions of the spinal cord in which the convulsive movements cease during sleep, that is, whilst the cerebrum is in a quiescent state; but at all other periods mental emotions are alone capable of inducing the convulsive action. In hysteria, an excellent example is afforded of the potency of mental emotions in the production of deranged reflex actions of the spinal cord. In illustration of the morbid irritability of the cord, a result of prolonged mental depression, I bring forward the following case:—

"A gentleman, aged 27, for three years suffered from severe mental depression, the result of various causes; his general health had been excellent, but for some time he suffered from bronchitis. To so low a condition were his mental powers brought, that he dreaded self-destruction; dark despair seemed to have taken up its fixed abode in his unhappy breast; his sleep became disturbed, and soon almost entirely vanished; he became most excitable at night, chiefly from want of sleep; he complained of occasional wandering pains between the shoulders, in the lumbar region, and down the extremities. It appeared evident to me that this state of things could not long exist without the manifestation of some morbid action or disease. On Friday, January 11, 1855, he got diarrhœa; the stools were chalky; this ceased, and on January 13th the

bowels were stated to be irregular, extremities cold, pains in the back, loins, and lower extremities; on January 15th some irregularity of bowels and coldness of the lower extremities; pain complained of in the back, lumbar region, and lower extremities; loss of appetite; pallor; sleep very much disturbed, and of short duration.

"January 20th. The symptoms last reported have increased in intensity; a feeling of numbness and tingling in the lumbar and hypogastric region, penis, perineum, and lower extremities, particularly in the balls of the great toes, is complained of, and also distressing tinnitus aurium; bowels constipated.

"23rd. The numbness is worst in the lower extremities; and with it a feeling of constriction is complained of in the perineum, penis, and hypogastric region. Since the last note there is great uneasiness felt in the rectum, and its *mucous coat is prolapsed;* the abdomen is full, tense, and tympanitic. Bowels rather loose to-day; fæces clayey; legs and feet very cold; tongue coated; thirst and headach are complained of. Eight grains of mercurial pill were given, and followed by black draught.

"25th. An aggravation of all these symptoms seems to have taken place; the tinnitus aurium is complained of as most distressing; the loins and lower extremities are numb and cold; the patient, although walking about, and engaged at his usual occupations, has still a want of confidence in his lower extremities, and often fears lest he may fall in the street.

"February 2nd. Symptoms still the same, and the numbness is confined to the same places; bowels are constipated; fæces are at some periods quite white, at others, tarry; urine ammoniacal and clouded.

"March 20th. No change in the symptoms. A new symptom is to-day complained of, viz., a constricted feeling below the umbilicus, as if a string were tightly tied about the abdomen. This sensation, which is one of the most constant symptoms of a morbid condition of the spinal cord, is caused by a deranged action of the diaphraghm, the floating ribs being drawn by its muscular contraction towards the vertebral column; the abdomen below the constricted line is full, tense, and tympanitic; the legs, from the knees down, are numb and very cold, and sometimes affected with "cramp;" a peculiar bursting sensation is felt in the soles of the feet, and great pain across the instep; the urine is ammoniacal, and on cooling it deposits a most copious *white* deposit, which Dr. Frazer, of Dublin, examined under the microscope, and found to be the triple phosphate of ammonia and magnesia; so copious was this deposit, that I could with ease have collected ounces of it. For a considerable period this state of things continued, the patient able to be out at his engagements, but spiritless and debilitated; yet he was not at all emaciated. Some slight improvement was beginning to manifest itself, when, at the end of three months from the onset of the attack, he got scarlet fever, during which all the symptoms

disappeared, but they quickly returned, with the single exception of the state of the urine, which ceased to deposit phosphates. About this period the mental depresssion began to disappear, its causes being removed; and finally, under a course of quina, and the local application to the lumbar region of the firing-iron, he completely recovered.

"Here was an undoubted case of spinal irritation, *excited* by pure mental depression; and I must say I feared, if not checked, ramollissement of the cord would have ensued. How truly does this verify those practical remarks of Watson, that 'where ammoniacal urine is coexistent with phosphatic deposits, some profound fault in the nervous system may be suspected; and it is a fact of practical importance that the tendency to alkalescence of the urine from fixed alkali, and therefore to phosphatic deposits, goes along with general debility, and signifies that the health has fallen below par. Persons who are jaded and spent through over-much toil, *whose vital energies have been depressed by mental anxiety*, by insufficient nourishment, or sensual excesses, are apt to pass water that is alkalescent, or but faintly acid, and to exhibit in their urine the tokens of the so-called phosphatic diathesis. They are for the most part cachectic, sallow, languid, spiritless, exhausted.' In such cases quina, in my estimation, ranks high; under its employment the normal healthy action is soon restored."

Dr. W. C. Townsend, Assistant Physician to the Cork Union Hospital, exhibited an interesting pathological specimen of *Cancer*, the particulars of which are as follows:—

"Jeremiah Ring, aged 50, labourer, was admitted to the medical ward of the Union Hospital, October 3, 1857; he complained of weakness and inability for exertion; was ordered to get a purgative draught, also a pint of porter.

"On my visit the following day, he complained of piles; on examination, I directed him to be transferred to the surgical ward. The hemorrhoids having been relieved, he left the hospital, November 9.

"On the 24th of November, he was again admitted to the hospital, with pain in the epigastrium, which he complained of having felt three days previously; was a good deal debilitated, and presented an emaciated, sallow appearance; a blister was directed to be applied to the epigastrium. On the following day he was ordered meat diet, with porter. On the 1st December he took a sudden dislike to food of any kind, with the exception of milk; he lingered on, complaining of the pain in the epigastrium, which continued without any interruption, and for which he was ordered repeated blisters, and opiate applications to the scrobiculum cordis. On the 30th of December he complained for the first time of nausea; this was relieved by a few effervescing powders; he never vomited. He died January 29, 1858.

"*Autopsy*, 28 hours after death.—Emaciation not considerable;

on section of the abdomen the liver was found immensely enlarged, in fact a mass of cancerous deposit; on opening the stomach, the entire of the lesser curvature was covered with scirrhous deposit; the pyloric orifice and inlet were quite free from disease; no unhealthy appearance in the lungs, pancreas, or kidneys.

"This case is interesting as evidencing the extent of disease that may be present without causing much inconvenience. This poor man left the hospital on the 9th of November apparently well; he returned again on the 24th of the same month, and died on the 29th of January following, within three months. Is it probable that so large an amount of disease could have been deposited within so short a period?"

February 24th.

The President, Dr. R. Corbett, in the Chair.

The Secretary read the following cases, communicated by Dr. Furlong, of Kinsale.

"Some time about the year 1816 or 1817, I was called upon in the course of dispensary duty to visit a woman some miles distant from my residence, in her confinement.

"On my arrival at the cabin, I found her, as was usual, lying on a bundle of straw, surrounded by female friends, amongst whom were a couple of that respectable class called *grannies*. She was about 35 years of age, the mother of three children, and I was informed had been in labour for two days. I observed the pains were sharp, coming on every five or six minutes, but not resembling the usual pains of labour. The pulse was not accelerated, neither did the countenance flush, nor did the abdominal muscles appear engaged. On examination I could barely feel the os uteri still closed. I concluded the pains were adventitious, and, being a young practitioner, felt rather angry at what I considered to be an unnecessary ride. I ordered an anodyne, to be followed by an oil draught, and desired to be sent for when real labour came on, if necessary. In about a fortnight after I was again summoned to her, and was told that after the anodyne the pains had ceased, but that she was now in good, active labour. On my arrival I found the pains very sharp, but in every respect as before. I now made a more careful examination, and found the uterus unimpregnated, the os closed and easily tilted on the finger. I examined the abdomen, and discovered in the right side that I could distinctly feel the form of a fœtus, and on further inquiry was told that the woman had her catamenial discharges all through her pregnancy.

"I now looked upon the case as one of extra-uterine gestation, the ovum being probably stopped in its passage, and its growth carried on in the Fallopian tube. I again recommended the use of the anodyne to be repeated whenever symptoms of labour appeared, explaining to the people, in such homely language as they could understand, my ideas of the nature of the case, and that probably in

a few weeks a circumscribed tumour with redness would appear, which they were to poultice; and left general directions as to her strength being supported.

"In about six weeks after, I was informed that an abscess had discharged, throwing out parts of a fœtus. Some portions which I saw appeared to be of five or six months' growth. Suffice it to say, the entire mass came away gradually, and the woman recovered her strength, supported by good diet, bark, and wine.

"This poor woman died in about four years after, I heard, rather suddenly, having only complained of some pain for a few hours, resembling colic. I did not see her at or after death.

" As *Traumatic Tetanus* is one of those diseases which too often baffle our best endeavours, the following case, which occurred while I was a student, fell under the care of a fellow student long since dead, and myself, and as recovery took place from a formidable train of symptoms, it may afford some interest to our medical brethren.

" This case occurred in Dublin, I think about the year 1811. A young woman, about eighteen or twenty years of age, servant to a housekeeper in a nobleman's family, having been much fatigued by night attendance on her mistress during illness, lay down at an early hour in the morning, with her back to the fire. She fell into so profound a sleep that she did not awake until her clothes were in a state of ignition from coals having fallen from the grate. On rising up they burst into flame, and the poor girl rushed out and threw herself into a large house cistern, which extinguished the flame, but not until she was extensively burned; the injury extending from the neck to the nates, and across the back from shoulder to shoulder, as well as across the loins.

" For the purpose of exciting nervous energy, my friend, who visited her immediately, poured a large bottle of spirit of turpentine over the burned part, which certainly aroused the sensibility of the parts, after which the whole burned surface was smeared with linimentum calcis, followed by poultices. Suppuration came on, but accompanied by trismus, soon followed by general tetanic spasms.

" My friend and I got a cork between her teeth, and commenced rubbing the jaws and neck with a strong anodyne liniment, giving at the same time a grain of solid opium, combined with two grains of extract of jalap, every hour, and placed a pillow under the abdomen, which, as she was obliged to lie on her face and hands, in some degree relieved the spasms, now amounting to opisthotonos. We continued the treatment for twenty-four hours, and then sought the advice of the late Dr. Hartigan, then Professor of Anatomy at Trinity College, whose lectures we were attending. He kindly saw the case, approved of the treatment, but assured us recovery was hopeless. We persevered, and had the satisfaction of seeing the symptoms yield gradually, enabling us to give our doses of opium at longer intervals, until all danger was over.

"The injured parts sloughed freely, and were perfectly healed in about two months.

"When Dr. Hartigan was, in a subsequent part of his course, lecturing on tetanus, he asked to have the young woman brought to the lecture-room, where he exhibited her to his class as one of the very few cases of severe tetanus he had ever seen saved.

"The following case may appear interesting, as exhibiting some of those anomalous symptoms occasionally resulting from *vitiated bile, or obstructed flow through the ducts.* In this instance the peculiar state of the circulation, until spasm or obstruction was removed, appears more particularly to claim attention.

"I was called upon, about fifteen years ago, to visit a clergyman who was largely engaged in farming business, aged about 50; sallow countenance; dark eyes, of peculiar depth; confined habit of body, and the frequent subject of hemorrhoids. I found him sitting up in bed, apparently much alarmed; free from pain of any kind, but every couple of minutes seized with convulsive movements of the eyelids, slight snuffling of the nose, and spasmodic twitches of the face, falling back at each attack on his pillow, but immediately resuming the erect posture. He was perfectly clear in intellect, spoke well, complained of nothing more than the violent stroke of his heart, which shook the curtains of his bed; the tongue was perfectly clean, and he had taken some purgative, which had not acted at the time of my visit. He had been standing, a day or two before, for some hours superintending labourers, in a marshy piece of ground.

"Believing the symptoms arose from biliary obstruction, I at first gave an emetic, which produced no amelioration of symptoms; I then applied a large blister over the epigastrium, and gave an active dose of calomel and colocynth, followed every hour with a wine-glassful of black draught.

"While anxiously waiting for the action of these medicines, I from time to time felt his pulse, which generally during the attack beat 28 strokes in a minute, and found it occasionally reduced to 13 beats; and at one time I actually counted 28 seconds between two pulsations; this occurred after one of those convulsive attacks I mentioned, and alarmed me considerably. The purgative having been taken some time, I immediately threw up a strong purgative enema, which acted speedily, bringing off a large quantity of black matter of a melanotic character. The enema was repeated soon after, with a similar result, when the convulsions ceased; the pulse rose to 60, and became steady; the tongue, hitherto morbidly clean, now became coated with a thick white crust, and he was well.

"I was sent for about two years afterwards to see him, and found him dead; he staggered while watching labourers at work, and fell; he was brought dead into his house."

Cancer of the Cardiac Orifice of the Stomach, and Hypertrophy of the Pancreas.—Dr. Finn narrated the history of this case, having

previously exhibited pathological specimens illustrative of the above lesions.

Catherine Finn, aged 60, was admitted into hospital on the 19th August, 1857, suffering from frequent vomiting and obstinate constipation. She stated that her habits were always regular and temperate, and that she had enjoyed uniformly good health up to about eight years since, when she suffered from obstruction of the bowels, which yielded to appropriate treatment. After this period her habit of body had been most regular for several months. About four years since she first suffered from an empty straining, which continued more or less throughout the day. After this had persisted for some months, vomiting supervened, some hours after taking food, accompanied by tenderness in the epigastrium. This symptom generally ceased or abated when the stomach had rejected its contents. The vomiting occurred most frequently during the night. These symptoms continued with more or less intensity till about six months previous to her admission to hospital, when rejection of food from the stomach immediately succeeded deglutition. The pain in the epigastrium had now ceased, and was replaced by a sense of soreness, experienced during the act of deglutition. Symptoms on occasion of admission to hospital:—Constant irritability of stomach; obstinate costiveness (this symptom demanding the almost daily use of lavements for the long period during which she was under treatment); tongue clean; pulse 74, small and hard. There was no fulness in the epigastrium; nor was there any intolerance of pressure over this region. On applying the stethoscope over the abdominal aorta, a loud murmur was occasionally heard, about an inch below the ensiform cartilage, and a little to the left of the median line. During the latter months of her illness she complained of some deep-seated obstacle to swallowing, referred to the interior of the stomach, and emaciation now progressed so rapidly that life was only sustained by the administration of enemata, composed of small quantities of beef-tea and arrow-root. Death took place on the 15th of February.

Autopsy.—Body wasted to an extreme degree; abdominal walls collapsed, and in contact with the spine. The liver, which was atrophied, presented a congested appearance and a diminished consistence. The other viscera were unusually pale. The stomach was remarkably small; its walls were much attenuated; and the interior was covered by a thin coating of greenish matter, on the removal of which the mucous membrane exhibited an anemic appearance similar to that of the other viscera. The pancreas was hypertrophied throughout its entire extent. The cardiac orifice of the stomach was both hypertrophied and indurated; a small body, about the size of a grape, was attached by a very short pedicle to its posterior aspect. The pyloric orifice did not present any deviation from the normal state. Sections of the cardiac orifice and of the grape-like process attached to it, examined under the microscope by Professor Purcell O'Leary, exhibited in a marked manner the form of

cells characteristic of cancer. The pancreas, having been subjected to a similar examination, did not appear to participate in the cancerous degeneration.

Remarks.—The occasional development of a murmur in the abdominal aorta interposed some difficulty in the way of an early diagnosis in this case; but the immediate rejection of food consequent upon deglutition; and the patient's consciousness of the existence of some impediment to this function, at or near the cardiac orifice, implied that th · gastric symptoms had their origin in some structural change involving this part.

Dr. Cremen communicated to the Society that within the previous month several cases of *Asthenic or Latent Pneumonia* had come under his notice, in which the characteristic phenomena of the disease were either absent or obscured by the general febrile condition; the patients complained only of lassitude, chills, and loss of appetite; there was no heat of skin; the pulse was slightly accelerated; in some of the cases the pleura was involved, and then pain was referred to the corresponding side; the left lung was oftener affected than the right, its base in all the cases. The following, which was the first that occurred, and in which Dr. Cremen at first overlooked its true nature, in some degree illustrates the character of the disease.

Pat Walsh, aged 30, a mason, presented at the Clarence-street Dispensary on the 8th of February, 1858, complaining of the above symptoms, which, he stated, he had laboured under for the previous five or six days, during which he worked at his trade. On the 7th February he was attacked with pain in the left side which disabled and obliged him to seek advice; he stated he had a cough so slight as not to signify; his aspect was peculiarly typhoid on this day; however, Dr. Cremen did not make a physical examination of his chest. On the 9th February he visited him, and found the symptoms the same as on the previous day; but on inquiry was informed that the sputa were discoloured and reddish; this caused him to suspect the existence of a lung affection, and on percussing his chest he discovered two-thirds of the left side dull, and the lung completely solidified to this extent. Bronchial respiration and bronchophony were audible over this space, and cough elicited a fine crepitus; there was no dyspnœa; the tongue was foul and creamy; the pulse was slightly accelerated, and feeble. The treatment consisted in a large blister to the side, a small quantity of calomel and opium, a combination of carbonate of ammonia and camphor mixture. Under this he gradually improved. There was some difficulty in convincing him as to the danger of his condition; and on one occasion, after exposure to cold, he relapsed, with an aggravation of his symptoms; he eventually convalesced favourably, but slowly. Dr. Cremen has seen him since, when he complained of debility and the sense of constriction at the seat of his former disease; he is now completely recovered under the use of tonic medicines and sulphate of quina.

The other cases were exactly similar in their history and symptoms: the treatment was purely stimulant and tonic, with quina, no calomel and opium. No one of the cases would warrant the abstraction of the smallest quantity of blood; one of the cases was fatal; the subject of it, a labourer, persisted in following his work until purulent infiltration of the lung took place. Beside these cases, there were some few cases of passive congestion of the lung, and two cases of highly congestive bronchitis, in which the sputa were sanguineous; the physical signs of pneumonia were absent. Dr. Cremen was informed of the occurrence of other cases of this latent solidification of the lung in the better ranks of life during the same period. Dr. Corrigan, in the Dublin Hospital Gazette[a], and M. Sancerotte, as quoted in the Medico-Chirurgical Review[b], have well described this disease. The latter states it may be confounded with pleurisy. The differential diagnosis of these diseases is too easy to admit of error when a careful examination is instituted. Dr. Cremen found that in some of the cases referred to, when pleuritic pain was complained of, there was not any difficulty about the diagnosis on physical examination.

The following case was reported by A. H. Orpen, M. R. C. S.:—

J. C., aged 35, a scavenger, admitted into the Cork Union Hospital under the care of Dr. W. C. Townsend, December 11, 1857, from the A B division of the workhouse, which he entered on the previous 1st of November. Before his coming into the workhouse he had undergone a good deal of hardship and privation, not having enough to eat and drink, and what he had was of an inferior description; he was also exposed to cold and wet; and was in the habit of drinking raw spirits. On admission to the hospital he complained of a dull pain at the pit of the stomach, which he stated he first felt a few days previously, while taking a drink of cold water; pulse 80; tongue furred; great thirst; appetite ravenous; no evidence of enlarged liver or spleen. Ordered a draught of castor-oil, with 30 drops of tincture of opium in some peppermint water, and to have a turpentine stupe to the pit of his stomach.

December 14th. He complained of sickness of stomach; ordered to have an emetic immediately, and two blue pills and colocynth at bed-time.

16th. Feels better, but the pain in the stomach comes on a short time after eating; ordered two galbanum and colocynth pills every night, and a draught containing two drachms of Epsom salts, with 15 minims of dilute sulphuric acid, every morning; also to have a blister to the epigastrium.

17th. Vomited four hours after dinner last evening; from this to the 28th he vomited at least once every twenty-four hours; on one occasion he did not vomit for six-and-thirty hours.

29th. Pain in stomach very severe; omit the pills and draught;

[a] Vol. ii. No. 12. [b] January, 1847.

ordered 10 grains of Dover's powder immediately; to be repeated (if necessary) at bed-time; also repeat the blister to the epigastrium.

30th. Pain not better; vomited twice yesterday, and once this morning. Ordered to get mercury with chalk, and Dover's powder, of each 5 grains, every fourth hour.

January 1st, 1858. The pain still continues. Omit the powders, and repeat the blister; tincture of opium, 25 minims, at bed-time.

7th. Has vomited once every day since the 1st; no relief from the pain; is much distressed by flatulent eructations. Ordered to get, every second hour, carbonate of magnesia, 8 grains; tincture of opium, 5 minims, in a little peppermint water; also to have an opiate plaster applied over the stomach.

20th. He was ordered to take, three times daily, 5 grains of the trisnitrate of bismuth, with 10 grains of the compound powder of tragacanth.

February 1st. He now vomits after every meal; pain at scrobiculus cordis still continuing, of a dull aching character, he was ordered milk diet exclusively; and to get 2 minims of dilute hydrocyanic acid, in a draught, three times daily; and a belladonna plaster over the stomach.

8th. Vomiting is still going on; he vomited matter this morning which consisted chiefly of milk curd, some of which also passed off.

10th. This morning the vomited materials consisted of a dark-coloured fluid, like coffee-grounds; is in great pain; a great deal of mental anxiety about him; pulse 84; tongue furred, very red at tip and sides; abdomen flatulent; very much emaciated; countenance assuming an olive, waxy appearance. Omit the hydrocyanic acid; ordered to take half a wine-glassful of lime-water three times daily; and one grain of watery extract of opium at night.

11th. No vomiting since yesterday.

12th. Vomited last evening, and again this morning; the pain still continuing; no tenderness on pressing the abdomen; no enlargement of any of the abdominal viscera can be detected; did not sleep last night; the vomited matter presented the same coffee-ground appearance.

15th. Is in great pain to-day; does not like the milk. Ordered to have a mutton-chop daily.

16th. Vomited yesterday, one hour after eating the chop.

18th. Coffee-ground discharge from stomach again to-day; dejections (which up to this time were watery discharges) rather feculent.

He lingered on in great agony, emaciated to the most extreme degree, and vomiting a few hours after eating anything. He died March the 5th, 1858.

Autopsy, twenty-six hours after death, by Mr. Orpen.—Emaciation extreme; his bones barely covered; and his face so emaciated that it appeared more like a skull over which a piece

of parchment had been drawn tightly, than anything else; the abdomen was sunk in, and the abdominal muscles were rapidly decomposing. On section of the abdomen, the intestines were found to be in a high state of decomposition; the stomach was found to be very much contracted, and adherent to the spleen; the liver was not enlarged nor contracted. On cutting into the liver it was found to be a good deal congested; no other alteration was observed in it. The spleen was enlarged and indurated; its fibro-serous investment was very much thickened and opaque, and was intimately adherent to the stomach, also to the diaphragm and abdominal walls. On cutting into the spleen, its convex surface, underneath the peritoneal sheath, was found to be invested with fibro-cartilaginous tissue, of bony consistency, to the extent of four or five lines in thickness; its parenchyma was highly congested, of a reddish-brown, sarcomatous appearance.

On cutting longitudinally through the coats of the stomach, it was found empty; its internal surface was covered with viscid mucus; on washing this off, the mucous and submucous areolar coats were found to be greatly hypertrophied; some slight redness was observed near the pylorus. The lining membrane of the duodenum, near the pylorus, presented a slate-coloured appearance; the pancreas was a mass of scirrhous deposit. Both the kidneys were found, on section, to be affected with Bright's disease; some carcinomatous deposit existed in the left kidney, which was larger than the right, and was adherent to the lower portion of the spleen.

Remarks by Dr. Townsend.—"Throughout almost the entire of this case there was constant vomiting, and yet the pyloric outlet was free and open; no bar to the passage of the contents of the stomach into the duodenum; no ulceration or trace of deposit in the stomach. True, there was great thickening of the mucous and muscular coats, but no ulceration; no apparent cancerous deposit; no impediment either at the inlet or outlet; still the patient was never free from pain. In contra-distinction, as evidence of the immense power of accommodation that organs are capable of, while disease slowly but surely advances, I exhibited a short time ago to the Society a pathological specimen, where the liver and the lesser curvature of the stomach presented a mass of specific malignant disease, and yet the patient never vomited; the pyloric orifice was also free from disease. In the former case the patient was never free from pain, vomiting almost daily, and, towards the end, after each meal; in the latter case there was no vomiting, and little pain or uneasiness, the marked symptoms being an inability to take food, and consequent wasting.

"Might the constant vomiting be owing to the adhesions that existed between the stomach, pancreas, and spleen? I am inclined to think so."

Case of Abdominal Aneurism.—Dr. Finn exhibited a pathological specimen of the above disease.

John Quade, aged 40, a labourer, was admitted into the North

Infirmary on the 14th December, 1857. He had been formerly a coachman, but for some years has worked as a labourer; his habits had been intemperate; latterly, however, declining health has compelled him to indulge to a less extent than previously. His general health had been good to the commencement of his present illness, and he never required medical treatment save for an attack of syphilis, from which he suffered several years ago. His present illness dated from about eighteen months since, having been ushered in by pain in the umbilical region, across the loins, and along the course of the sciatic nerve. Has lately suffered much from flatulent distention of abdomen after eating, and from obstinate costiveness. Has also suffered much from coldness of the lower extremities.

Present symptoms: Anxious expression of countenance; tongue clean and moist; appetite bad; costive state of bowels; pulse 76, regular; complains of dull pain in the left hypochondrium; this pain, which is much increased towards evening and night, precludes continued sleep. Within the past few days suffers from an occasional paroxysm of pain of a burning kind, so intense as to cause him to moan, and lasting for an hour or two; it shoots from the umbilicus to the right inguinal region; decubitus on left side; dorsal decubitus occasions a sensation "as if the heart were flying out of him." He suffers occasionally from vertigo when walking rapidly or working more severely than usual. In the left hypochondrium was discovered a tumour of about the size of a small orange, which, viewed from a short distance, resembled an abscess about to open externally; this tumour at its highest point of elevation (the centre) was raised a little more than an inch above the surrounding surface. It communicated a strong impulse to the hand at every point of its surface; but no murmur was heard on applying the stethoscope, except at the distal extremity of the tumour, near the umbilicus, where the superficial elevation of the skin abruptly terminated. The murmur at this point ceased in the erect position. The sounds of the heart were normal.

December 30th. Slept better than usual; pulse 66, equal in both wrists, but feeble. From the 14th to the 30th December the tumour rapidly increased in size, measuring more than 5 inches transversely, and $4\frac{1}{2}$ inches in the vertical direction. It now extended beyond the mesial line to the right side. From the latter date to the period of his death on the 23rd February, his sufferings progressively increased, and relief was only obtained by the use of anodynes frequently administered.

Autopsy.—General emaciation. The epigastric tumour, which had previously existed, had wholly disappeared. The cavity of the peritoneum contained a very large quantity of blood. Some blood was also observed to have insinuated itself in several places behind the peritoneum, reflected on the viscera. The blood escaped from a vast aneurism, situated behind and between the crura of the diaphragm, and extending thence to within about an inch from the bifurcation of the aorta. On the anterior surface of the aneurism, and above

the cœliac axis, there existed an opening of irregular form, about an inch wide, through which the blood escaped into the abdominal cavity. The distal end of the sac overlapped the uninjured portion of the vessel, interposed between it and the bifurcation. The aneurismal sac was with great difficulty separated from the spinal column, to which it was closely adherent. The bodies of three of the lumbar vertebræ had suffered erosion. The liver was atrophied and pale, and appeared to be displaced upwards. The pericardium contained about three ounces of serum; in other respects the thoracic viscera appeared healthy.

Remarks.—The absence of murmur in this instance may be referred to the vast size of the aneurism; the murmur heard at the distal side of the sac existed in a portion of the vessel free from dilatation. In an obscure case of abdominal aneurism recorded by Dr. Hope[a], who had an opportunity of making a post-mortem examination, he attributed the absence of murmur to the lateral inexpansibility of the sac. The reverse of this condition of the sac having existed in the present case, with a similar result, suggests that much yet remains to be accomplished in this field of inquiry.

Case of Foreign Body in the Air-Passages. By JOHN WILLIAMS, M.D. —"From the character of the symptoms that are attendant on the accidental passage of a foreign body, such as a solid or fluid particle, into the windpipe, there is rarely difficulty experienced in recognising the nature of the accident, if recent. The same remarks are applicable when *complete* occlusion of the trachea or right or left bronchus is caused by a piece of meat, or, as in a case given by Dr. Stokes, in his work on Diseases of the Chest, by a peeled kidney bean, which so completely obstructed the right bronchus as entirely to prevent respiration in the corresponding lung; but in those cases in which all the immediate urgent symptoms have subsided, and those which are present indicate rather morbid action either in the bronchial mucous membrane or in the parenchyma of the lung itself; the recognition of the presence of a foreign body in the air-passages, under such circumstances, is, in my experience, a matter attended with a vast amount of difficulty.

"In the case which I purpose to bring before the Society this evening a like difficulty presented itself, and at no period was I afforded any means whereby I might correctly determine the presence of the foreign body. Various rules are laid down in systematic treatises, whereby a correct diagnosis may be arrived at, but in practice it will, I am sure, be found impossible invariably to make application of those rules. When a foreign body, with a perfectly smooth surface, and of such dimensions as not seriously to affect the passage of air through the air-tube in which it is located, has got by accident into the lung, if not soon expelled, irritation either of the pulmonary parenchyma or of the bronchial mucous membrane is quickly ex-

[a] On Diseases of the Heart, page 462.

cited; the foreign body forming in one case the nidus of an abscess, or in the other it remains *in statu quo*, but has given rise to inflammatory action in the bronchial investment. Now if the foreign body be of such a nature as a pea, or a portion of soft fruit, &c., that it may be softened and mixed with mucous, be expelled without attracting the notice of the patient,—the inflammatory action, notwithstanding the removal of the cause, may be persistent. This is ordinarily the case in persons that are of a strumous diathesis; but if the foreign body be of such a nature as an iron nail, tooth, cherry-stone, or such like, its presence is without difficulty detected. I recollect having heard the present Professor of Anatomy in Belfast College, Dr. Carlisle, a profound physiologist, when Lecturer at the Park-street School of Medicine, declare that 'a pea by accident got into his trachea; that for three days or more it caused intense irritation; but since then he had never *heard from it*.' He did not to his knowledge expel it; so I think it probable it became soft, and thus escaped notice in expectorated mucus. In all cases the severity of the symptoms is directly proportioned to the nature and dimensions of the foreign body. Of course it must be allowed that the constriction of the tube itself (owing to muscular contraction) does exercise an influence on the ingress and egress of air, but this must be trivial in amount. In the case which I now bring forward, the foreign body, which was evidently in the right lung, did not altogether interfere with respiration, but, like a vibratory tongue in a musical instrument, modified the natural respiratory murmur, and occasioned the stridor. As to the position that it must have occupied, there can now be no doubt, from the universal bronchitis that immediately ensued.

"In the early part of October, 1857, I was sent for to visit Mrs K., who, I was told, was suffering from cough with expectoration for nearly three months. The history she gave me of her case was that in the month of July—when she was in excellent health—whilst eating peas, which were rather old and tough, at dinner either a whole one or part of one passed into the windpipe: this was followed by a violent and prolonged fit of coughing, during which her face became highly congested, and ultimately, when almost exhausted from the violent exertions to get rid of the foreign body, she coughed up a portion of the skin or spermoderm of the pea. For some days she 'felt something loose in her throat, which moved up and down, and caused her great inconvenience.' This sensation soon disappeared, but she fancied she got cold at the time, as 'the cough never ceased since.' Her respiration quickly became hurried and stridulous, and it caused her great distress to walk up stairs. She went to the country, and was under medical treatment, with the particulars of which I am unacquainted; but at the expiration of nearly three months after she 'got cold,' as she herself thought, she for the first time came under my notice and care. When I saw her, she was much emaciated. Physical examination revealed most extensive bronchitis over the entire chest; the

right lung was rather more affected than the left, which fact was also known to the patient, for she referred most of the symptoms complained of to that side of the chest. After the least exertion her respiration became hurried and stridulous. She was therefore wholly confined to the house, and usually sat for hours leaning forward, and resting her elbows on her knees, as that position gave her most ease. The expectoration, which was sero-mucous, was very profuse; her pulse was weak and quick; she had no desire for food, and her countenance wore an anxious expression. The dyspnœa and stridulous respiration assumed a paroxysmal character both in the day-time and at night, and, while present, assimilated closely spasmodic bronchitis or asthma. I must here remark that the physical exploration of the chest revealed to me no other signs than those attendant on ordinary bronchitis, and I was therefore inclined to refer the present attack to cold caught at the time the accident occurred; but, from what cause soever it originated, a certain group of signs and symptoms presented themselves to me, and I therefore shaped my plan of treatment accordingly. With a view to unload the bronchial tubes, I ordered an emetic of ipecacuanha to be given daily. This was pursued for a few days with good effect, so far as relieving the dyspnœa, but it then produced such active purgation that I was reluctantly obliged to discontinue it. I now began the senega with the carbonate of ammonia, and counter-irritation to the front and back of the chest alternately, ordering at the same time a generous diet. For some time I witnessed no change of any amount in the urgent symptoms, and at each visit I generally found my patient with a countenance expressive of great anxiety, whilst the lips were purple, and the conjunctiva deeply congested. Notwithstanding, I never, during the whole course of my attendance on this lady, observed any departure from the signs and symptoms that ordinarily accompany a severe attack of spasmodic bronchitis. I now began the quina treatment, with an occasional draught of camphor mixture with Hoffman's anodyne at bed-time, still continuing the acetic acid and turpentine liniment externally, and I soon witnessed a steady improvement in all the symptoms. She began now to gain flesh; her pulse diminished in frequency, the dyspnœa became less urgent, and she was enabled to take walking exercise. She continued to improve steadily, and I considered my attendance on her no longer necessary. On paying a friendly visit some time after, she expressed herself nearly as well as ever, but occasionally, after walking up stairs or other exertion 'she suffered, as she expressed it, from quick breathing and wheezing,' but her appetite and general health were nearly altogether restored, and likewise her spirits, which previously were much depressed. At the end of four months after my first visit, or seven months from the date of the commencement of the attack, I was informed, when attending Mr. K. for an attack of inflammation of the ankle, that the evening before Mrs. K. was suddenly seized with a violent fit of coughing which lasted more than an hour. During this paroxysm her friends

feared it was but to herald something worse, her face became so congested,—when, to the surprise of all, she coughed up what proved to be a pea, and quickly after she coughed up the skin of it, into which it fitted accurately. From this period not a trace of either dyspnœa or stridor remained, and in order to complete this report I called on her last Saturday, and found her in her usually good health. However, as I anticipated from the severity of the bronchitis which pervaded both lungs, she is rather liable to catch cold, and is therefore obliged to be cautious."

March 16, 1858.

Dr. R. Corbett, President, in the Chair.

An extra meeting was held on this evening to afford an opportunity, not only to the members of the Association, but also to their medical brethren, of observing the phenomena so remarkably exhibited in the person of M. Groux, for which purpose this meeting was thrown open to the profession at large, when that gentleman explained the several points worthy of observation; but as the case of M. Groux has been already fully described, we shall not here enter into its peculiar features.

Dr. S. Henry Hobart then brought forward the man George Sinick, who had been operated on for *Carotid Aneurism* in September last, and whose case has already appeared in the February Number of this Journal (1858); the man appeared to be in good health, and complained of no uneasiness in the site of the former aneurism. There is evident deficiency of the sterno-mastoid at the left side, that muscle having been twice divided during the treatment of the case; still, though there is some tendency to turn the head to the left, he is well able, and generally does, keep it quite straight.

March 24, 1858.

Dr. R. Corbett, President, in the Chair.

Dr. Cremen exhibited a specimen of *Uterine Hydatids*, and detailed the following case:—

Ellen Mitchell—aged 40, a slight, delicate, cachectic-looking woman, mother of thirteen children, ten of whom died very early; the last was premature, at seven months, and occurred two years ago; the accident resulted from violence; states that the catamenia have been always regular, but absent for the last four months. She did not think that she was pregnant, when, on the night of the 9th of March, she was seized with violent and recurring pains in the lumbar and hypogastric regions; a month previous to this date she fell while carrying a heavy load on her head, when the abdomen came against the kerbstone, since when she has suffered at intervals from pain in the lower part of the abdomen. After suffering

for some hours from the pains on the 9th March, some large fleshy masses were expelled; these Dr. Cremen did not see. On visiting her on the 10th March she was tranquil and free from pain; the abdomen was enlarged to about the usual size at the third month of utero-gestation; the uterine tumour was well defined, but wanted the firmness of healthy pregnancy; exquisite pain was complained of on pressure over the hypogastric region; the pulse was 96; other functions natural. She was ordered anodynes, with turpentine fomentations, after which the symptoms subsided, and she went about her *duties* in a day or so.

On the 21st March she was again seized with uterine pain as before, and after some hours a large fleshy mass was expelled; this was followed by considerable hemorrhage. On visiting her on the 22nd, Dr. Cremen found her blanched and feeble; and on instituting a vaginal examination the os uteri was soft, moist, and dilated to about the size of a two-shilling piece; the finger seemed to move over a fleshy surface like placenta, with a feel corresponding; this specimen was expelled on the 23rd March, and from this period she gradually recovered. It is necessary to observe that the patient had watery discharges from the uterus for seven or eight days previous to the 9th March. Dr. Cremen would consider the diagnosis of uterine hydatids as dependent, among other circumstances, on the following:—

1. The age and cachectic condition of the patient; the fact of her having had many pregnancies, some of them abnormal.

2. The absence both of mammary development and sensation of quickening at or after the usual period these phenomena manifest themselves.

3. The extreme flaccidity of the abdomen, not in conformity with the firmness of healthy pregnancy.

4. The occurrence of watery discharges.

5. The peculiar sensation given to the finger in vaginal examination, viz., soft, fleshy, and so extremely yielding, not corresponding with, nor to be mistaken for, any of the fœtal presentations, except, perhaps, that of the abdomen; the absence of hemorrhage also precludes it from being mistaken for placenta.

6. The absence of the fœtal heart-sounds at the usual time; also the result of a superficial examination of the abdomen, for the purpose of ascertaining the uterine contents, whether it may contain a dead fœtus, or a mass of these abnormal growths.

Cases in Obstetric Practice. By William Cummins, M.D., Medical Officer, Blackrock Dispensary.—"On the 8th of last November I was engaged to attend Mrs. ——, aged from 30 to 35, in her first confinement; she was then in the ninth month of pregnancy, and I was informed that she had suffered much during the entire period from gastric irritation, accompanied by constant acidity of stomach; that these symptoms had increased of late, and that enormous swell-

ing of the lower extremities, with a rash over the entire body, had been superadded within the last few weeks.

"I immediately paid her a visit, and found her in the following distressing condition:—

"The skin is completely covered with a papular eruption, which is a constant source of torment, and prevents sleep, by the irritation it occasions. The normal plethora of pregnancy has been increased tenfold by the anxious but injudicious care of her attendants, who have been constantly forcing the richest and most nourishing food upon her unwilling stomach, with a view to supporting her strength against the hour of nature's trial.

"The skin is hot, the pulse quick, the tongue somewhat coated; the abdomen, which is enormously enlarged, is supported by two limbs of colossal magnitude; so great indeed is the swelling that the thighs cannot be approximated; there is besides an erysipelatous blush over both legs, and the bowels are habitually costive.

"I commenced my treatment by countermanding all orders for nourishment, and prescribing the blandest possible diet, with saline purgatives; then, as I believed the rash to be principally occasioned by absorption of the excess of acid in the stomach, I ordered alkalies, and in a day or two followed these measures up with a gentle mercurial and diuretics, and an occasional opiate at bed-time.

"The unpleasant symptoms were soon palliated, but the swelling of the lower extremities continued, with only slight decrease, until labour set in, which it did on the morning of the 13th, with trifling pains, which continued during the day, and effected a moderate dilatation of the os uteri.

"From the first, however, the vagina was extremely tender, hot, and dry, so that I had to keep her more or less under the influence of tartar emetic. Towards evening the vagina could scarcely be touched, so great was the tenderness; the pulse also became quick and hard, and the skin very hot. I then took from eight to twelve ounces of blood from her arm, and increased the dose of tartar emetic; she lost a little more blood during the night from the bandage getting loose. Towards morning the pains, which had never been very good, gradually declined, and soon ceased entirely; the abdomen became tympanitic, and enormously enlarged, and she could not lie for a moment in any position except on the back: but with all these unpleasant symptoms I was gratified to find the soft parts relaxing, and that the tenderness, heat, and swelling had almost entirely left the vagina. I now had the benefit of Professor O'Connor's valuable advice and assistance, who coincided with me in thinking the case a most alarming one, and that delivery ought not to be attempted immediately; we gave her a full dose of laudanum, hoping to compose her for a time.

"After this she obtained a little sleep, and continued in much the same state, but without uterine action, until 4 P. M., when we administered a drachm of ergot in divided doses. This caused slight

pains, which dilated the os to about half its full size, and brought the fœtal head just within the brim of the pelvis, when they ceased.

"At 7 P.M., the os being dilatable, I applied the long forceps, without difficulty, in the transverse diameter of the brim, and delivered the head; the shoulders remained within the uterus, and it was not without great difficulty, delay, and the use of the blunt hook that the arms were delivered one by one. The child, which was very large, was lost. No uterine contraction followed the birth; and so smart was the hemorrhage that the placenta had to be removed from the uterus, and large doses of brandy given before any contraction occurred.

"Convalescence, assisted by the free use of opium, and an occasional mercurial, proceeded wonderfully well; the rash did not reappear, and the swelling of the limbs gradually subsided; in short, we took our leave at the usual time, our patient expressing herself pretty well. A day or two after, I paid her a visit, when she informed me, for the first time, that ever since her confinement she had suffered great pain during defæcation, although she had perfect control over the motions.

"I had made particular inquiries from the nursetender, during the days subsequent to delivery, as to whether the soft parts were all right, as I had not satisfied myself on that point immediately after the birth, the alarming flooding having demanded our entire attention, and no persuasions being able to induce the lady to submit to my making an examination afterwards. She now, however, admitted the folly of such false delicacy, and with great hesitation allowed me to examine, when I found that the perineum was lacerated as far as the anterior edge of the sphincter ani, and that the vagina and vulvæ were much inflamed. I prescribed a mild mercurial each night at bed-time, gentle saline purgatives, and injections of warm milk and water into the vagina. In about a week the inflammation had nearly subsided, when I commenced cautiously the introduction of candles into the vagina, gradually increasing the size each day; at the same time I applied nitrate of silver freely every second day to the edges of the laceration. This practice was eminently successful; and after a few weeks the laceration had cicatrized as firmly as if sutures had been used; the inflammation of vagina and vulvæ had also ceased without leaving any adhesion or other untoward consequence.

"During utero-gestation the system is in an intermediate state between health and disease, and nature is obliged to adopt prophylactic measures, to prevent the necessary physiological condition of overaction or plethora from running into the pathological. The natural sympathy between the uterus and stomach, by which the latter is rendered intolerant of excessive nourishment, is the partial closure of nature's floodgate against repletion of the vascular system. In the case I have brought forward a train of suffering and most alarming disease was the consequence of art interfering with this wise

provision of Providence; and it is an error into which ignorant people are continually falling, believing that as two beings are to be supported, a double supply of food is necessary, forgetting that the woman is created to support her offspring, and that it is only when a second being is to draw nourishment from her womb or breasts that the menstrual blood can be retained in her system without injury.

"It is fortunate that we do not often meet with such complications as this case presented, for if powerful uterine action coexisted with the condition of the vagina and vulvæ I have described, most appalling laceration would certainly have occurred; I believe firmly that this lady's life would have been sacrificed, or the rest of her days would have been spent in loathsome misery, far worse than death, had not the lancet been used. There was no time for relieving the plethora and inflammation by any less direct means; it was one of the many exceptions to the general rule that 'the blood is the life.' Before bleeding, the gentlest vaginal examination produced such agony that the patient's shrieks could be heard far from the house, and more than once I had to give over the attempt as impracticable. After bleeding, I was able to pass the blades of the long forceps, and draw the head through it without much suffering. What an answer to the absurd doctrine, so prevalent in these days of hydropathists and homœopathists, that venesection is never justifiable, much less necessary! I will now turn to the great delay and difficulty which occurred between the birth of the head and shoulders.

"It will be remembered that when the long forceps were applied, the os uteri was *only about half dilated, but dilatable.* Now it has occurred to me (though I find no mention of it in authorities), that although it is very easy to mechanically dilate such an os to the fullest extent, and draw a full-sized fœtal head through it, *while the mechanical dilating force is still holding open the lips of the womb*, yet that the dilating force having been removed, a reclosure of the lips may impede the passage of a much smaller part, unless it be *sufficiently wedge-shaped* to effect a dilatation for itself, which I think the shoulders may not be in many cases, and certainly are not when the inferior part of the body has been delivered, and the shoulders, with the arms upraised, remain within the uterus, as in the following case:—

"Mrs. Pead, dispensary patient, mother of three still-born and two living children, when daily expecting her sixth confinement, was standing quietly in her bed-room when the membranes suddenly gave way without any pain; the liquor amnii in large quantity escaped, and immediately after the arm of the fœtus was protruded between the vulvæ. This occurred on the 4th of November last, at 8 P. M. I was called to her at midnight. There had not been a trace of labour pain, though the entire forearm was hanging out between the thighs; I immediately introduced my hand, found the os imperfectly dilated, but dilatable; turned the child, and brought down feet. I then paused, and gave her a drachm of ergot in divided doses;

it produced no pain of any moment. Fearing the fœtus might be lost, I now drew down the feet gently, fully expecting to be able at once to complete delivery, but, just as the umbilicus had passed the external parts, and the shoulders were engaged in the right oblique diameter of the pelvic brim, it stuck fast, and no extractive force that I could use advanced it one step further. Of course in a short time the fœtus was lost, so that I had time to pause an hour or so, hoping for uterine action, but none came on. I now set to work resolutely to deliver, and first endeavoured, as I had done before the child's death, to hook down the limbs with my fingers, but it was impossible. I then passed the blunt hook over one shoulder, and after several unsuccessful attempts succeeded in drawing it down until one arm escaped from the uterus; then it was easily drawn out of the vagina with the finger. I had to repeat the same process with equal difficulty for the other arm. I then perforated the head, and having fixed the crotchet firmly, finished these troublesome operations by delivering the head. Strange to say, though there had not been a labour pain from first to last, there was little or no hemorrhage. Convalescence was rapid, so much so that the patient was sitting up by the fire (not with my consent) in a week.

"If the loss of the child, and all the trouble and difficulty which occurred in this case, were not caused by imperfect dilatation of the os uteri, I confess I am at a loss what to attribute it to, for the child was not larger than usual; and although I think there was some slight contraction of the pelvic brim, yet certainly it could not have been enough to impede the *shoulders*, as the patient had previously brought forth living children at the full time. Further, the position of the long diameter of the shoulders in the largest diameter of the pelvic brim was the most advantageous possible. Then there were no tumour or other impediment, so that, unless we admit the possibility of atmospheric pressure as the cause, it must have been imperfect dilatation of the os uteri which detained the shoulders. It will be said that the shoulders ought easily to pass through the os, which admitted my hand. True, if they were as capable of dilating it; but I deny that they are, for, independently of their being essentially passive, whatever little wedge-shape they may have when presenting at the brim *after the head*, they certainly have none when presenting the other way with the arms raised.

"But, to conclude, what practical points are we to derive from these cases? I fear, none in the first, except the satisfaction of knowing the cause of the difficulty; but, in cases similar to the second, a most important one, viz., having turned the child in the uterus, to wait until uterine action sets in and naturally expels the child, instead of endeavouring to complete the delivery at once."

TRANSACTIONS OF THE BELFAST MEDICAL SOCIETY[a].

SESSION 1857–58.

(*Continued from* p. 249.)

April 6, 1858.

The President in the Chair.

Mr. Browne gave the following account of a case of *Amputation at Hip-joint*:—

"The subject of the following brief remarks first came under my notice on the 1st of February, when I took charge of my wards in the General Hospital for the Spring Session, and when I obtained the short account of the case I now beg to submit to the Society.

"H. C., aged twenty-one years, of strumous appearance, though possessing considerable muscular development, in the month of July, 1857, sustained a simple fracture of the left femur, at the junction of the upper and middle thirds, by falling over a cask on the quays at Liverpool; for this injury he was admitted into the South Hospital of that city, where he remained for nine weeks; at the end of that time he was discharged with the fracture united, and he was able to walk pretty well, and without pain, by the assistance of a crutch. At the time referred to, there was considerable swelling around the injured part, and which remained stationary in size. He continued to move about, though still unable to work at his trade as a 'moulder,' till near the end of November, when one day, walking along the street here, he made a false step, and 'perceived,' as he expressed it, 'a severe jerk in the seat of the former injury;' smart pain immediately after ensued, and he felt necessitated to remain in bed. In about a fortnight or three weeks after, when turning on his couch, he felt the fractured part again give way, and at once experienced inability to move the limb.

"On the 24th of December he was admitted into the accident ward of the General Hospital under Dr. Murney; at that time there was complete mobility in the fractured part, but without any eversion or shortening of the limb; and there was a large hard mass about the seat of the injury, which was considered then to be an excess of the callus which in the reparative process had been thrown out. The limb was then carefully bandaged, and a Liston's long splint applied, extension and counter-extension being made in the usual way. During the six weeks which elapsed before I had charge of the case, the only visible change that occurred was the slow, progressive enlargement of the swelling at the point of fracture, which led to the belief that some disease of the bone had taken place.

[a] These Transactions are furnished to us by Mr. Browne, President of the Society.—Ed.

"On the 5th of February I took down the limb, and then carefully examined the affected part, and at once came to the conclusion that not only was there a want of union, but that considerable disease of the bone existed. Up to that time, I may remark, the patient had suffered scarcely any pain in the part; but on the occasion of the examination referred to, considerable handling and movement having taken place, he experienced a great deal of suffering for three or four days, during which time the tumour visibly increased, especially in front towards the groin. It is worthy also of note, that till this time, and when the patient learned that there was some disease of the bone, his general health had been excellent; appetite good; the circulation tranquil; and sleep natural.

"Between the 8th and 14th of February several consultations were held by the attending and consulting surgical staff of the Hospital, when, although some difference of opinion as to the nature of the disease took place, there was, with one exception, an unanimous admission that the only chance for the poor fellow's life was amputation of the limb at the hip-joint. With regard to the views entertained relative to the character of the disease, the generality inclined to the belief that the affection was then one of a non-malignant nature, an opinion which I strongly entertained from the characteristics of the tumour, the history and progress of the case, and the general aspect of the patient.

"Previous to the examination on the 6th of February the swelling felt almost solid, without any signs of softening at any point; but at the period of the second consultation on the 10th, the tumour had not only increased considerably in size, but, upon pressure, conveyed to the touch a sensation of fluctuation produced by a deeply seated fluid, and this sign, I may here observe, was increased whenever the swelling was much moved or handled. At this time it was deemed judicious to explore the tumour by puncture; accordingly, I introduced into the most prominent point of it a fine exploring trocar, for some three inches in depth; from this puncture some blood alone flowed, and that very freely, as if from a large vessel or sac containing blood; about an ounce was permitted thus to escape, when the puncture was closed by a strip of adhesive plaster. This fluid was immediately placed in the field of the microscope by my colleague, Dr. Murney, when nothing but healthy blood globules were discovered. That was on the 14th of February. The circumstances of the case were then freely explained by me to the patient, and that his only chance, and that not a good one, rested on the operation at the hip-joint; and, in justice to myself, I must say that I did not speak by any means encouragingly of the result. The poor fellow, however, after some consideration, determined to take his chance, and I then made preparation to operate with all practicable safety.

"On the day preceding that appointed for the operation it was considered advisable by my colleagues, who attended the final consultation, to make a still further exploration by means of an inci-

sion and the introduction of a long canula; this was done, and a canula, eight inches in length, was introduced by a small incision through the integuments; through this, fluid blood, which coagulated quickly and firmly, flowed quite freely; about four ounces were allowed so to escape; and on moving the canula about gently, it came in contact with the bone, which was felt to be rough and spiculated. On removing the canula, and closing the incision by means of a strip of plaster and compress, the swelling felt softer, and the sensation, to the touch, of fluctuation became more distinct. On the following morning, at half-past 9 o'clock of the 17th February, assisted by my colleagues, I proceeded to operate. That morning my patient had been prepared by taking an egg beat up with milk, and two ounces of brandy, and previous to his leaving the ward he had another ounce of spirits. Having been placed upon the table, and brought under the influence of chloroform, he was put in the proper position, and my assistants assigned their several posts. From the size which the swelling had attained upwards, encroaching on Poupart's ligament, and which increase had been very rapid for several days, it was considered advisable, to avoid probably diseased structure, not to make the anterior flap, in the usual way, by transfixing and cutting from within outwards, but to make a flap of the integuments, and then divide the muscular structure by a circular sweep of the knife. All things being prepared, and the circulation in the femoral vessels fully commanded by pressure made upon the anterior iliac artery, which was ably done by Dr. Pirrie, I made a semicircular incision through the integuments by a rapid sweep of the knife; this incision commenced over the edge of the pectineus muscle, and terminated a little outside of the anterior spine of the ilium the greatest circumference of the flap being about eight inches from Poupart's ligament. This flap was raised by a few rapid touches of the knife, and perfectly healthy muscular structure exposed; having placed the point of my finger on the femoral, which was almost laid bare, and finding that the circulation in it was completely controlled, I at once cut right down upon the hip-joint and through the adductor muscles. The instant the knife made this incision an immense gush of blood took place, evidently from a large cavity which contained some pints of that fluid, and from the vessels of the limb below the incision, very little being lost *directly* from the femoral vessels, as Dr. Moore most promptly seized them, and as Dr. Pirrie still had command of the circulation. At this stage of the operation it was considered the safest step at once to secure the femoral vessels; consequently, ligatures were rapidly placed both upon the superficial and deep arteries, and also on the femoral vein. On then feeling for the head of the bone, to proceed to disarticulate, I found that the entire bone had disappeared from the point of fracture to the acetabulum; I at once, therefore, passed the knife behind the point of the remaining bone, and cut a large-sized flap from the back of the thigh: in this incision only one or two twigs, of no importance, were divided, the knife entering the soft parts far below the trunk or main branches of either

sciatic or gluteal arteries. As the patient was now evidently greatly weakened by the loss of blood and shock, and as considerable venous oozing was going on, I applied a strong solution of matico to the face of the stump, which I then closed, and firm pressure was made over it by the hands of assistants, while restoratives and stimulants were exhibited to the patient, so soon as the diminished effect of the chloroform permitted him to swallow. After some time he was removed to a heated bed, and warmth was applied by means of hot-water pans placed around him. Beef-tea and stimulants were administered at intervals, as the stomach could bear them. The shock to the nervous system appeared to produce the greatest effect, for although considerable heat of body was restored, and the pulse greatly improved, he laboured under that depression which often follows severe injury without any loss of blood. Up to 8 o'clock in the evening he seemed likely to rally, but about a quarter to 9 greater depression ensued, and he sank at half-past 9 o'clock, twelve hours after the operation.

"*Remarks.*—The first matter to which I shall refer in my remarks upon this case is the nature of the swelling. I have already stated, that upon the first deep incision being made, an immense gush of fluid took place, as from a cavity that had been filled by it; but besides this a very large quantity of coagulated, or, what appeared coagulated, blood was detached from the cavity or cyst; this coagulum was at some points intermingled with lymph-like matter, and which I believe was lymph; this coagulum amounted to about four pounds.

"On examining the face of the stump, both the front and posterior flap, but especially the latter, presented a lining membrane precisely like the usual pyogenic membrane, which had lined the cavity in which the mass of fluid and coagulum which escaped from the limb had been lodged, and this membrane was peculiarly well developed around the part where the femur had been broken, and for some distance down the thigh, in a couple of pouches, one on either side of the limb. The bone was completely absorbed between the point of original fracture and the cartilage of incrustation on the head of the femur; this latter I picked out of the acetabulum after completing the incisions.

"Before the operation, some difference of opinion arose as to whether the tumour was one of the encephaloid kind; my own opinion was in the negative, and in this I became confirmed after the examination I made by puncture the evening preceding the operation, and also from the rapid increase of the tumefaction upwards which took place during the night, and whence I was led to believe that the tumour was composed almost or entirely of blood; and the more especially when I remembered a case of acute necrosis of the femur in which I amputated at the trochanters, and in which, on the evacuation of a seeming abscess, situated on the inner side of the thigh, the fluid was found to be composed principally of blood, and where the sac filled again with blood in the course of a few hours after the first evacuation. On removing the limb in this case of

necrosis, we found a large cavity along the inner side of the thigh, which was lined with a true pyogenic membrane, and it was this which had filled with blood shortly after the fluid it at first contained had been evacuated; this blood, we found, had been poured out from vessels that had become ulcerated in the progress of the disease. Something of a similar kind occurred, I am satisfied, in the case under consideration, as the swelling increased so rapidly after the limb had been moved about, and after the exploratory punctures; and at the time of the operation this fluid was pushing its way up under Poupart's ligament, and extending behind the sheath of the femoral vessels. That the great mass of the swelling which existed was composed principally of blood altered and unaltered in character, there cannot be any question; and while I am not prepared to deny that the disease had assumed a malignant type—cancer-like cells having been discovered by some examiners under the microscope—I am clear that the growth did not present any of the characteristics of the encephaloid form of disease, as seen either in the soft parts or in bone; nor do we find in such growths the existence of the distinct lining membrane which was found in my case. In fact, as I said before, the mass consisted of blood unaltered in appearance, and of an oily red and grumous fluid, like what has been described as existing in osteomalacia. This grumous matter, Solly remarks, shows a cell-development, and is probably an adventitious morbid growth, and not simply fatty matter altered by the effusion of blood into it.

"At all events, the case is one of great interest: a healthy, sober man sustains a simple fracture of the femur; this unites, seemingly all right; four months after, he slightly injures the part by making a false step, not by any direct blow; and a few weeks after, the union of the fracture gives way while the patient is turning in bed; and five months after the apparent sound repair of the fracture, the bone, on amputation, is found to have entirely disappeared from the point of original injury up to the cartilage of incrustation.

"I believe we were right in attempting to save life by the perilous operation performed; for there cannot be any question but that the disease must have proved fatal within a very limited time. The nature of the tumour was a question of great obscurity, though its fatal tendency was one of equal certainty, and the appearance of the parts after amputation 'fully justified,' as one of my colleagues remarked immediately after the removal of the limb, 'the attempt made to save life, no matter what the issue of the operation might be.'

"The patient, as I have already remarked, sank more from the shock to the nervous system than from the loss of blood, and this shock, the effects of which are so much greater in some persons than in others, we are not able either to anticipate, control, or avert. Some of the members may be desirous to know the statistics of amputation at the hip-joint; therefore I beg to say that 127 cases have been operated on, and 77 have died. In 47 amputations after injury, 35 proved fatal; in 43, for chronic disease, 24 recovered, and 19 died. All the cases of hip-joint amputation in the Crimea, 12 in

number, died. I should say these statistics are taken from Mr. Erichsen's last edition of his Surgery, with my case added."

Professor Gordon stated that Mr. Browne should have noticed that he, Dr. Gordon, had dissented from the operation, on the ground that he had no doubts of the malignant nature of the tumour; and he wished it to be distinctly borne in mind that he had not been a consenting party, and had, consequently, not attended the two final consultations. He looked upon the complaint as one of true medullary sarcoma, and that any operation must prove worse than useless. He did not observe the existence of any membrane lining the cavity.

Dr. Murney remarked he had watched the progress of the case attentively, as it was one presenting many features of great interest. He believed, although there were several questions of considerable importance arising out of the history, and comparatively obscure progress of the patient, for the two months preceding the operation, yet there were two points on which he would occupy the attention of the Society for a few minutes:—1st. The nature of the disease; and 2nd. The propriety of operative interference. Before entering on these questions, he would refer to one remark which had fallen from the President, viz., "That the tumour on the upper part of the thigh was looked upon as a mass of callus." Such opinion he did entertain from the patient's account of himself at the time of his admission, but when the man had been some little time under treatment, this mass had attracted his suspicions; it was no longer looked upon as a healthy deposit.

So long as the patient considered he laboured under a simple fracture of the thigh, he slept well, had an excellent appetite, and was, in fact, getting fat; but immediately after the first exploratory examination, when he was informed the disease would certainly deprive him of his limb, and perhaps destroy life, the anxiety of his mind caused him to become sleepless and nervous, with loss of spirits; and from that time, too, his appetite failed; but at no period had he symptoms which, in Dr. Murney's opinion, would indicate the presence of malignant diathesis; on the two occasions on which explorations were made, the microscope indicated merely the presence of blood. After the removal of the limb, he had examined the mass which formed the tumour; during the operation it was fluid, and entirely of the appearance of blood. One quarter of an hour afterwards there was a number of coagula, of the consistence of an imperfectly formed clot, surrounded by fluid, the mass appearing to be blood, with a number of fibrinous-looking spots and streaks scattered through it. On microscopic examination with a power of 300 diameters linear, the greater portion of the mass was blood; the fibrinous-looking material attracted considerable attention; and lest there might be any mistake, he had examined several specimens on three or four different occasions; on each of those a number of cells was seen presenting the usual appearance of such structures found in malignant growths, but re-agents certainly did not produce the effects noticed in such cases; for instance, there was no increase in the size of the cells on application of water, nor dimi-

nution on addition of syrup; acetic acid did not in any way alter the cell-wall or nucleus, and liquor potassæ did not produce the usual results. From these circumstances he considered the mass of the tumour contained nothing but blood and cartilage cells; the latter might be looked for as normal elements of the ossifying process at the site of a fracture. In their physical appearance, especially where met without the hyaline solid blastema, in which they are usually seen, as on articular surfaces, &c., they are not unlike cancer cells, and in some instances are only to be distinguished from them by the effects of re-agents. Finally, he was of opinion the case was to be looked upon as an example of the second form of sanguineous tumour of bone, as described by Mr. Stanley.

Referring to the second question, viz., the propriety of operative interference, Dr. Murney said the obscurity of the symptoms prior to the operation had not enabled him to make up his mind whether the disease were malignant or not; he had often stated he thought the weight of evidence was in favour of a non-malignant character; and if such were the case, he was sure all would agree that the patient's prospect would be favourable if he survived the shock of that most serious operation, amputation at the hip-joint. If the affection were malignant, however, not to operate was to leave the sufferer to certain death; and in the absence of symptoms of carcinomatous disease in any other situation, by removal of the limb, a chance for life, however remote, was given. Under all circumstances, he believed the operation was fully warranted. Bearing on this question, he observed, in the second volume of Mr. Paget's Lectures on Surgical Pathology[a], the following remarks, which he considered so directly applicable, he would take the liberty of placing before the notice of the members:—"A motive for operations in cases of supposed medullary cancers may often be drawn from the uncertainty of the diagnosis. This is especially the case with those of the large bones, for the removal of which the peril of the necessary operation might seem too great for the probability of advantage to be derived from it. I have referred to cases of cartilaginous and myeloid tumours of bone (pp. 181, 215, 219), in which, during life, the diagnosis from medullary cancers was, I believe, impossible. In all such cases—and I am sure they are not very rare—the observance of a rule against the removal of tumours, or of bones believed to be cancerous, would lead to a lamentable loss of life. All doubts respecting diagnosis are here to be reckoned in favour of operations."

The PRESIDENT begged to say that he was quite willing to admit that Professor Gordon had opposed the operation; but he must remind the Professor that there were four opinions against his, and each of which was as freely expressed as his own. He felt quite satisfied about the existence of the pyogenic-like membrane to which he had referred, and he also insisted that the great mass of the tumefaction was composed of blood. He was happy to hear the clear views entertained by Dr. Murney, and he fully agreed with all the remarks which he had made regarding the case.

[a] Page 410.

An Historical Critique on the Cure of Popliteal Aneurisms by Digital Compression; with reference to several Communications made to the Société de Chirurgie. Drawn up by AR. VERNEUIL, Fellow of the Faculty; Surgeon to the Hospitals.

SINCE the publication of the valuable work of our excellent friend, M. Broca[a], the question of the treatment of aneurisms has been reopened, and compression has resumed a pre-eminence it ought never to have lost. Perhaps this is, to a certain extent, to be attributed to the legitimate crusade waged with so much ardour against the surgery of the knife. However this may be, the Société de Chirurgie has latterly had the good fortune to receive three reports of aneurisms cured by compression performed with the fingers.

We had lately announced the communication of M. Vanzetti, of Padua; at the same time, a work by M. Michaux, of Louvain, containing a similar case, reached the Society: so that we shall, without further delay, proceed to bring the subject before our readers.

With reference, first, to the new facts: it occurred to M. Vanzetti, who had seen compression employed in Dublin in 1843, to substitute, for the mechanical means there used, the hand of several successive assistants. In 1846 he made a trial of this mode at the Hospital of Kharkoff, in Russia, conjointly with M. Serebriakoff, surgeon to the hospital. He himself instructed the persons intrusted with the maintenance of the compression as to the place and manner in which it should be employed. The compression was kept up for two days, but without effect. The patient was operated on by ligature.

Eight years later, in 1854, M. Vanzetti received under his care in hospital, at Padua, a mason, aged about 36 years, of tolerably good constitution, labouring under a well-marked, though medium-sized, popliteal aneurism.

The pulsation ceased as soon as the femoral artery was compressed. M. Vanzetti resolved to employ compression; but, discouraged by the failure just alluded to, he tried a great number of compressors, with much patience on his part, and much inconvenience to the sufferer. Before resorting to tying the femoral artery, he wished to try digital compression once more, in the hope that, under his immediate superintendence, it would prove more successful. Several assistants—seated or standing, sometimes with one hand, sometimes with both hands placed one over the other—compressed the artery with *a very moderate force*, sufficient to bring the walls of the artery together, but without inconveniencing the patient. The situation chosen was the middle third of the thigh, in order that the profunda artery should not be obliterated.

At the end of twelve hours there was considerable diminution of the expansive movements of the tumour. At the end of forty-

[a] "Des Anévrysmes et de leur Traitement." Paris: 1856.

eight hours there was no pulsation, nor could any bruit be heard and the compression was withdrawn. There was no relapse. The tumour in the ham gradually disappeared, and, at the same time, the leg became straight again. This man has resumed his occupation; he walks without limping, and experiences no inconvenience in the movements of the knee.

The second case is still more remarkable. In 1855, a very intelligent officer, aged 27, consulted M. Vanzetti for a popliteal aneurism of the right side. The employment of digital compression was resolved on, but was deferred for twenty days. During this period the patient himself made compression, the action of which had been explained to him. The tumour, which was of small volume, seemed to diminish even under the influence of these imperfect attempts.

The real treatment was commenced at noon, and was confided to six intelligent and reliable assistants. At 6 o'clock, M. Vanzetti returned to see what state things were in. The patient was fast asleep; not one of the assistants was with him. Great was the surgeon's surprise; greater still was his astonishment when he learned that after FOUR HOURS of careful compression, *the aneurism had become still*, and that its pulsations had completely ceased! Accordingly, at 5 o'clock, that is, an hour after, the assistants had withdrawn. The patient remained at the Clinique for a month. He was seen again several months later; there was no lameness, and the movements of the limb were free. The aneurismal tumour was converted into a solid nucleus of the size of a filbert.

Such are the facts furnished by the Paduan surgeon, and which he has put forward with the most remarkable clearness. M. Marjolin, in his turn, has communicated two cases reported by M. Michaux, of Louvain, one of the most distinguished of foreign surgeons. The first is a long chirurgical drama, commencing with double and intermittent compression, and ending with amputation of the thigh, rendered necessary by gangrene of the limb after ligature of the femoral artery. This case is one of great interest; but as it does not bear directly on the point under consideration, we shall not further allude to it. The details may be found in the Bulletins of the Society.

The following is a succinct analysis of the second case:—

A man, aged 50, of good constitution, who had always enjoyed excellent health, complained, for the first time in 1851, of rheumatic pains in the left thigh. In August, 1856, the patient discovered, at the anterior and inner part of the thigh, a pulsating tumour, situated nearly two and a half inches from the crural arch. Its progress was rapid, for in the month of November it measured five and a half inches in its transverse and four and a third inches in its vertical diameter. It presented very distinctly all the signs of aneurism. The complete examination of the patient denoted a general alteration of the arterial system; abnormal sounds toward

the centre of the circulation, and a rather rough souffle in all the large arteries. The other functions were tolerably well performed.

What treatment was to be adopted? If ligature was to be employed, it should be applied to the external iliac, always a serious operation, especially when the vessel is altered. On the other hand, alternating compression with two pads was not compatible with the very high situation of the tumour. M. Michaux decided on making single compression on the ilio-pectineal eminence, with the small pad of M. Broca's apparatus. The treatment was commenced on the morning of the 28th of November; the compression was to be intermittent, that is to say, every four or five hours the apparatus was to be loosened to allow the patient to rest. The next day, the severity of the pain rendered it necessary to suspend the compression for four hours. On the 30th, a vesicle had formed on the surface of the compressed skin; the pressure was again suspended for four hours. The pad was placed a little lower; compression was alternately suspended and re-established, until the 2nd of December, as the presence of vesicles indicated the formation of an eschar.

During these four days the pain had been extremely severe, especially in the loins and in the heel. There was complete want of sleep, notwithstanding the employment of opium; the patient was very much fatigued, and nevertheless the tumour had diminished in size, and had become hardened; on several occasions, when the permanent compression had been prolonged for four or five hours, the pulsations had disappeared. M. Michaux then thought of using digital compression; by good fortune, a portion of sound skin remained within the eschar, where the application of the finger was sufficient to arrest the pulsations. The pupils of the hospital applied themselves to the task with the greatest zeal, on the 5th of December, at half-past 9 in the morning; the compression was to be total; the pulsations of the artery being energetic, tolerably strong pressure was necessary to obstruct the vessel.

Compression was very painful on account of the vicinity of the eschar. In the afternoon, œdema, tingling, numbness, and rigidity of the limb were present; the feet became cold; there was complete want of sleep, as well as acute thirst, intense fever, and violent colic, at 2 o'clock in the morning; but the tumour became perceptibly solidified; the pulsations diminished; soft clots filled the sac. The local improvement continued without interruption; at 9 o'clock in the morning, twenty-four hours after the establishment of the digital compression, the pulsations and the bruit de souffle entirely ceased. The arteries no longer pulsated below the aneurism, but above it, on the contrary, the commencement of the femoral pulsated strongly.

As a matter of prudence, the compression was kept up until 2 o'clock in the morning; it was then interrupted for half an hour, to afford some respite to the unhappy patient, after which it was resumed until 9 o'clock in the morning, to be finally given up.

I stop here to impress more forcibly the mind of the reader, and

I pass in silence over the details of the general treatment, which was very ably conducted during all this period, as well as during convalescence. On the 14th of December the eschar separated; it involved the entire thickness of the skin. On the 16th of January the wound had cicatrized. On the 24th the patient left the hospital perfectly cured. The aneurismal tumour was very hard; it is still of a tolerably large size (two inches by two and three-fourths), but the cure is no longer doubtful.

Did they stand alone, the remarkable cases just reported would suffice to commend digital compression strongly to the attention of surgeons. What objection can there be to a proceeding which cures popliteal aneurisms in five hours—even in forty-eight hours; a considerable aneurism of the femoral in twenty-four hours? What, without digital compression, would have been the fate of M. Michaux's second patient? Let us observe that, in two cases, no resource remained but the ligature; the insufficiency of compression by means of apparatus was palpable. If M. Broca had had these facts before him when he was preparing his book, he would certainly have modified the chapter he has devoted to the operative proceeding we are speaking of. Without tracing, step by step, the history of digital compression, we may be permitted here to discuss with our learned friend some of the propositions he has announced in his book.

While he considers digital compression to be "a very valuable resource," M. Broca regards it as a means which we must reserve for the following cases:—1. When the deviation of the limb does not permit us to act efficiently on the artery by means of mechanical apparatus. 2. When excessive irritability of the skin is opposed to every other species of compression.

Now, the latter condition alone existed, and in but one of the three cases above quoted. The two other cures show that it is advisable to enlarge the scope of the process. On further consulting the facts collected in M. Broca's book, I find another very remarkable case, that of Mr. Greatrex. The alternating employment of a compressor *and of the patient's fingers* put a stop to the pulsations at the end of twenty-four hours.

In a case of Mr. Knight's, compression with apparatus could not be borne for more than an hour. Forty hours' continuance of digital pressure on the pubis was sufficient to insure a cure.

To crown the work, we have the curious case of the patient under Mr. Colles' care, who cured himself by keeping up for seven days a digital compression, which was necessarily very imperfect.

It is far from being my intention to conceal the tolerably numerous failures of this process. But, on the whole, what have we lost when we have failed? Nothing; even when the means employed prove unsuccessful, as happens also occasionally with compression by apparatus. It appears to me that digital pressure has been less painful for the patient than the latter, which may then be advantageously substituted for it.

For my part, notwithstanding the constantly progressive im-

provements of apparatus, which, according to M. Broca, render the cases very rare in which digital compression is indicated, I consider, with him, that it constitutes the type of the method, and that consequently it is only in extreme cases that we ought to draw back from the difficulties of its application. In common with M. Michaux, I can scarcely doubt that, in the immense majority of cases, we should be able to find the devoted co-operation of a sufficiently large number of assistants to take part in an enterprise as noble in a humane, as it is interesting in a scientific point of view. Hitherto digital compression has most frequently been employed only after mechanical compression. I think that, hereafter, we should proceed in exactly the inverse direction, and, when compression is indicated, commence with the pressure of the fingers before having recourse to a less gentle process.

Perhaps it will not be uninteresting to take a rapid survey of the cases of aneurism in which digital compression has been employed, whether alone or combined with mechanical pressure.

These trials, in number seventeen, include seven unsuccessful and ten successful cases. I shall add some explanatory remarks on the two categories[a].

FAILURES.—*Four times* compression was employed primarily—that is to say, before any other means. A. Popliteal aneurism; two days' compression; ligature (Vanzetti). B. Popliteal aneurism; four hours only, then mechanical compression for six days (Jameson). C. Diffused aneurism of the femoral; sixty-two hours; apparent cure, which proved not permanent; seven days' compression with a weight on the groin; decisive cure (Parker). D. Popliteal aneurism; digital compression maintained by convalescent patients for three days; mechanical compression, previously intolerable, was rendered possible; a cure was obtained (Monro, J.).

In two instances, digital compression was not employed until after the insufficiency of apparatus had been demonstrated, and their use abandoned:—E. Diffused aneurism of the ham; sixteen days of mechanical compression; ninety-four hours of digital compression, at the end of which the pulsations and the souffle had disappeared; the success was not permanent; amputation; death (Nélaton). F. Popliteal aneurism; employment of bad apparatus for five days; twenty-four hours of digital compression; ligature; amputation; recovery (Norgate). G. In this instance, both modes of compression failed, but the case was one of inguinal aneurism; compression with the fingers was kept up during four days and four nights; the tumour was greatly ameliorated; the tourniquet having been applied, subsequently produced an eschar; the external iliac artery was tied (Fox).

In these seven cases of failure we find three ligatures subsequently applied, A, F, G; and two amputations for gangrene, E, F.

[a] In M. Broca's work details relating to these facts will be found. I have given only a very brief analysis of them, distinguishing the cases by letters and the names of the observers.

Two ligatures were successful, A, G. One of the two patients subjected to amputation died. Three times, on the contrary, B, C, D, mechanical compression was followed by cure. Even when digital compression had to be abandoned, it modified the tumour advantageously in two cases, C, E. In a third, D, it singularly promoted tolerance of the apparatus.

Let us proceed to the—

Successful Cases.—*Two* belong to digital compression, employed primarily and alone:—H. Popliteal aneurism; five hours were sufficient (Vanzetti). I. Colles's patient, who, without assistance, cured himself in seven days of popliteal aneurism, by intermittent, and, of course, very imperfect compression.

Three times digital compression succeeded when mechanical compressors were inapplicable, or abandoned:—J. Femoral aneurism; mechanical compression for four hours; after twenty-four hours digital compression produced a cure (Michaux). K. Popliteal aneurism; various apparatus were tried for a long time, and were given up; forty hours of digital compression (Vanzetti). L. Apparatus intolerable; forty hours afterwards, the employment of the fingers; cure (Knight).

Four times the pressure of the fingers having been combined with the use of the tourniquet, it is difficult to award to each method its due share of merit. This method, which is equivalent to double and alternating compression, has further produced results so rapid and decisive that, perhaps, we ought to dwell on this combination, which would singularly relieve at once both the patient and the surgeon.

M. Popliteal aneurism; tourniquet below the groin; the patient's finger pressing on the pubis; at the end of twenty-four hours, cure (Greatrex). N. Popliteal aneurism; digital compression on the pubis; below, Dupuytren's compressor; alternating action, forty-eight hours; recovery. O. Same lesion; same mode of proceeding; sixteen hours were sufficient for the cure (Wood). P. The following case, by Mr. Tufnell, is more complicated; in it, however, we find a combination of several modes of compression—Very voluminous popliteal aneurism; compression, according to Bellingham's process, for two days; suspended on account of swelling of the inguinal glands; digital compression on the pubis by the patient and a neighbour; tourniquet acting inferiorly, and alternating with the action of the fingers; the whole kept up for twenty-four hours; the pubic weight[a] was then employed; cure at the end of seven days.

Lastly, in the following case, digital compression having been employed conjointly and alternately with apparatus and the direct compression of the tumour, it is difficult to assign to it a precise

[a] A conical weight of lead, varying in amount from four to nine pounds, as may be necessary efficiently to compress the artery, where it is superficially situated in the groin. See "Practical Remarks on the Treatment of Aneurism by Compression," by Jolliffe Tufnell, M. R. I. A., &c. Dublin: Fannin and Co. 1851.—Translator.

part in the case—Q. Arterio-venous aneurism of the bend of the elbow; direct compression on the tumour for several days, then indirect compression with the tourniquet, which could not be borne; digital compression kept up by the patient, and suspension of every other means; resumption of direct compression on the tumour; digital compression was made by assistants for twenty hours, and then abandoned; it was intrusted to the care of the patient alone; finally, some days after, the aneurism was cured (Nélaton).

We should have more than one remark to make on all these interesting cases, and, in particular, on the place where it is advisable to apply the fingers in cases of popliteal aneurism. Perhaps we should adopt the situation chosen by M. Vanzetti; but this discussion would carry us too far.

In the present state of the question, we may safely announce the following propositions:—

I. Indirect digital compression, continuous, and even intermittent, performed by the able hands of assistants, or by the patient, has succeeded alone, and without the previous or subsequent assistance of any other means, in curing aneurisms.

II. Associated with the tourniquet, and alternating with it, it has produced cures both rapid and extremely simple. In general, when success is to crown the attempt, it does not tarry.

III. Employed alone, it has cured aneurism when mechanical compression was impracticable, or had to be abandoned; much better borne, in fact, than the latter; digital compression may be applied on points where the skin is already inflamed.

IV. "This compression is the most efficacious and least dangerous of all; it enables us to act only on the artery, avoiding the nerves and neighbouring veins, and sparing the skin."—*Broca*, p. 807.

V. Digital compression may fail; but in this case it most frequently modifies advantageously the state of the aneurism.

VI. We are justified in believing that, alone, it would have succeeded more frequently if it had been practised with more perseverance and regularity than were employed in the above quoted cases.

VII. Never, so far, has any accident been attributable to this proceeding.

VIII. Having been applied successfully, for the first time, by Saviard, subsequently to an operation for aneurism after the old plan, indirect digital compression is essentially of French origin. Hitherto it has not received all the extension and generalization of which, in my opinion, it is susceptible.—*Gazette Hebdomadaire*, 30th October, 1857, p. 773.

Description of a Deformity of the Thorax, with loss of Substance of the Ribs; followed by Remarks on the Movements of the Heart. By Dr. FRICKHOEFFER, of Idstein[a].

BLUMDAHIER, son of a master cartwright, a boy of fourteen, labours under a deformity, consisting chiefly in a very decided hump and a lateral curvature of the spine, accompanied by a deviation of the ribs, some of which are imperfect.

The inclination of the vertebral column commences about the middle of the cervical region, is directed at first from left to right and backwards, then almost transversely; and finally, directly from above downwards, thus forming the figure of a turned Roman S.

The greater convexity of the thorax corresponds, therefore, anteriorly to the right half, posteriorly to the left half. The right side deviates inwards, the left outwards, and the greatest transverse diameter of the chest bisects the sternum, and answers to the inferior angle of the left scapula.

The following points, observed on the anterior aspect of the thorax, are worthy of remark:—

1. The sternum is wider than in the normal state, and is directed obliquely from above downwards, and from right to left.

2. Of the left ribs, the first alone is articulated to the sternum; the second terminates at the distance of two and a half inches from that bone; the third, fourth, and fifth descend at first almost in a straight line from the hollow of the axilla, and stop at three inches from the sternum; and the others, articulated to one another by a common cartilage, form from left to right a semilunar arch, and terminate at half an inch from a rudimentary xiphoid appendix.

3. In consequence of this malformation and of the absence of a portion of the ribs, there exists on the left side of the chest a sort of triangle with the base above formed by the first rib and the lower edge of the great pectoral muscle, the truncated apex being directed downwards; at the right superior angle is the nipple; the sides of the triangle are three and a half inches long, its surface is half an inch lower than that of the rest of the chest.

4. In this triangular space are situated the inferior organs of the thorax, covered only by the skin, beneath which the movements of the heart and of a portion of the left lung are distinctly seen.

5. The motions of the heart are observed at the upper angle of the space just described. A movement from left to right is plainly distinguished, and on palpation the impulse of the heart is felt immediately under the skin, appearing reduplicated, and consisting of two shocks, the one longer and stronger, the other shorter and weaker.

[a] It will be interesting to compare this paper, inserted in Virchow's "Archiv für patholog. Anatomie," Bd. x. Hft. 4, with that published by M. Béhier, in reference to Groux, who is affected with a malformation of the sternum, and in whom also the movements of the heart can be observed. See Archives Générales de Médecine, 1855.

The first is isochronous with the systole, the second follows the first and the systole, simulating a weak and rapid rebound.

The motions of the lung are easily recognised by an elevation and depression isochronous with inspiration and expiration, and by the edge of the lung covering the heart gliding alternately from left to right.

6. On percussion it is found that the heart extends from the place where the impulse is perceived to the right under the sternum, and downwards as far as the xiphoid cartilage, and that it has rather a transverse direction from left to right and from before backwards; consequently, it is probable that the part of the heart which pulsates in the triangle corresponds to a portion of the left ventricle.

Moreover, percussion in the upper angle of the triangle yields a clear sound over a circumference of two square inches, which, therefore, covers a part of the left ventricle, and occupies the greatest extent of the triangle; the inferior third furnishes a completely dull sound, and contains the stomach, as well as the left lobe of the liver.

7. Auscultation discloses in the free portion of the heart the two normal sounds, the first being longer, and being emphasized. In the rest of the precordial region the two sounds continue the same, although they are weaker on account of their distance. The accentuation is always laid upon the first, which extends towards the free part of the heart, and corresponds to the stronger impulse already mentioned. The second sound, which is shorter and weaker, corresponds to the second shock, which presents the same characters.

On the contrary, in the region in which, as would appear from the clear sound yielded on percussion, a portion of the left lung xists, a vesicular murmur alone is heard; in the lower third of the triangle, where the dulness is detected, the ear perceives no sound.

In considering this case, we should first bear in mind that it offers some analogy to that of congenital fissure of the sternum presented in the person of M. Groux, whose case has intensely interested the medical world, and led individuals skilled in auscultation and percussion to test its results. In the present case, also, there is a part of the heart covered only by the skin and pericardium, as well as portions of the lung, of the stomach, and of the left lobe of the liver; but instead of a solution of continuity of the sternum, we have an imperfect development of the ribs, and a much more considerable loss of substance of the left side of the thorax.

This anomaly, very striking in itself, acquires a greater interest by rendering it possible to observe directly the motions of the heart; if the latter power does not lead to new results, it is capable at least of refuting or confirming certain controverted opinions.

The first question to examine is this: *What is the part of the heart which we see and touch in the triangle above mentioned?*

We must remember that in consequence of the deformity of the thorax, by reason of which its right half deviates, and its entire cavity is compressed from above downwards and from before backwards, all the organs must be pushed to the left and backwards, the triangle being half an inch below the surface of the rest of the chest.

We should also recollect that the heart, in order to be able to follow, with the arch of the aorta and its descending portion, the curvature of the spine, must be situated higher up, and have a direction rather horizontal than vertical. Comparing the results of percussion, which give the heart a transverse situation from left to right, and from behind forwards, with those of auscultation, which enables us to hear, in the most distinct manner, at the free portion of the heart the two normal sounds, the first being stronger and more accentuated, the character, according to Skoda, of the sounds occurring in the ventricles, while the contrary obtains for those in the arteries, we can no longer doubt that the left ventricle belongs to the part of the heart in question, and probably to its upper third.

The only point on which any discussion could arise is, as to whether we had to do with a portion of the left ventricle, or of the trunk of a large vessel: the character of the sounds appears to me necessarily to decide the question, as I have done.

2. This case, in which we distinctly perceive the shock of the heart towards the free portion of the left ventricle, gives great weight to the opinion of Kiwisch, who admits that this shock does not depend on an impulsion of the apex against the thoracic walls, but on the perception, across the intercostal spaces, of the rigidity of the muscular fibres of the heart during the systole; for in our case the impulsion takes place only in the point corresponding to the left ventricle, consecutively to the systolic contraction, and is perceived in no other point of the thorax. The opinions of Bamberger on the impulse of the heart, in a well-formed subject, agree fully with our observation: the author just mentioned considers as obstacles to the perception of the rigidity of the heart during the systole, in the normal state, the lesser thickness of the right ventricle, which is situated immediately beneath the thoracic parietes; the greater approximation of the superior ribs; the increased thickness of the upper part of the chest; and thus expresses himself in Virchow's "Archiv:" —"I have often been convinced that in children and very emaciated persons, the impulse of the heart frequently takes place over a greater extent, and that it is often perceived in other situations than those against which the heart directly beats: let the muscles of the chest in a rabbit be removed to the level of the intercostal spaces, and we shall observe in each point touched by the heart the pulsation of that organ, even when it was not previously perceived."

In the case above described those obstacles do not exist; therefore the normal systolic contraction of the ventricle and the accompanying impulse are perceptible. It hence results that the cause of the shock does not reside in the impulsion of the point of the heart, but in the sudden contraction and rigidity of the two ventricles.

3. As we have seen, a double pulsation might be observed at the free part of the heart,—the one stronger and longer, the other shorter and weaker; the first isochronous with the systole, the second following the first, of which it appears to be a more rapid and feebler after-stroke; moreover, the former corresponds to the first sound of the heart, the latter to the second sound. This observation was so con-

clusive, and the concussion communicated to the hand by the second shock was so distinctly perceived, that no doubt could be entertained; and two medical students, who examined the case with me, satisfied themselves of the existence of this phenomenon.

Is it necessary to object that there is in our patient no affection of the heart, whether dilatation or hypertrophy, and that, therefore, our observation possesses no interest in reference to these lesions, as Skoda[a] thus expresses himself on the subject of double pulsation of the heart:—"In several cases of hypertrophy with dilatation of the two ventricles, the systole produces an impulse; the diastole by no means raises the thoracic parietes, but produces a concussion: the latter is not caused by the shock of the heart against the thorax, but appears to be isochronous with the retraction of the heart against the spine. This phenomenon is evidently the same as that designated by Laennec under the name of 'impulsion of the auricles.'"

The second shock observed here appears to me to be much more simple, and to have the same cause as the second sound, with which it is isochronous; namely, the concussion of the column of blood contained in the arteries after the ventricular systole, against the sigmoid valves, and this view is supported by the following reasons:—

a. The shock takes place in a space of time which corresponds to the end of the systole; it follows the first systolic shock, of which it seems to be a slighter rebound, and coincides with the fulness of the arteries and the closure of the sigmoid valves. There can be no doubt that, if the impulse of the arterial column is capable of communicating a concussion, this does not affect the whole heart, but only the part nearest the arteries; the shock is too insignificant to be perceived on the normal thoracic walls.

b. These researches had led me to this result, that the free portion of the heart corresponds to the upper extremity of the left ventricle. The second sound perceptible in this region might then be attributed, not only to the second impulse, but also to the ventricular contraction; but the authority of Skoda does not allow us to admit this hypothesis:—"We cannot assert that in a heart in the normal state the second sound is always produced in the ventricle, since it is very probable, if not certain, that the second sound heard over the heart occurs in the arteries, and that on account of its intensity, it is perceived at a certain distance; but there are cases in which we are obliged to admit the existence of the second sound in the ventricular region. Sometimes we do not at all, or only feebly, perceive the second sound above the base of the heart, while it is very strong and very distinctly observed at the apex"[b].

But in our case, the second sound does not exist at the apex, while it is very distinct and very strong at the base; it, therefore, evidently takes place over the sigmoid valves, and is propagated farther. The second impulse of the heart, which is isochronous with it, must, according to all probability, correspond to the same point.

[a] Treatise on Auscultation and Percussion, p. 152. [b] *Loc. cit.*, page 187.

We might here admit a recoil movement, contrary to Skoda's ideas, as has been done for the systolic impulse. This recoil is produced during the diastole, before the arrival of a new column of blood in the arteries, by that which is already contained in them, and which falls back upon the tense and closed sigmoid valves, on account of the elasticity of the arterial walls; it might, therefore, be termed a movement of recoil by elasticity.

If, on the contrary, the second sound took place in the ventricle, it would be heard at the apex, and not at the base, and we might, therefore, seek the cause of the second impulse in the shock of the blood against the ventricular walls during the diastole, if we accepted Skoda's hypothesis, that the shock of the column of blood against the ventricular walls, during the diastole of the ventricles, may sometimes produce the second sound[a].

As to the hypothesis which consists in comparing the movements of the heart to those of a lever, by which Hope explained the impulse of that organ, for which Volkmann lays down restrictions, but of which the truth is rendered very doubtful by the recent researches of Bamberger and Kölliker, at least in its connexion with the impulse of the heart, our case is, in my opinion, little suited to confirm it, for we can observe only too small a portion of the heart, and that of the base alone, and the other anatomical proportions of the chest, are too far removed from the normal state to warrant us in giving a definite judgment on this point. We can merely conclude from the result of our researches, that the supposition of a double force is not admissible, because in this case we should observe, during the systole and the striking of the apex of the heart against the chest, which immediately follows it, a retraction of the upper part of the heart, of the visible part of the left ventricle, as well as of the pectoral wall covering it, while, on the contrary, we observe a projection and connexion of the part of which we are speaking.

4. This case furnishes also some data as to the locomotion of the heart. The movements of this organ, which take place under the portion of the thorax deprived of ribs, demand for their appreciation close attention, because the movements of the heart and lung continually set in play the skin which covers them. But after long observation, we clearly established motions of elevation and depression, isochronous with inspiration and expiration, with a slight gliding of the edge of the lung; finally, an undulating movement of the heart from left to right and from above downwards, isochronous with the systole. This latter movement would be rendered still more evident by tracing with ink a transverse line over the precordial region. This locomotion corresponds, it will be recollected, to the assumed position of the great axis of the heart, and, in all probability, we ought not to consider the contraction and shortening of the ventricles, in their long diameter, as a cause of the inferior motion, an opinion which has very long been refuted; but, on the contrary, the prolongation of the column of blood during the diastole,

[a] *Loc. cit.*, page 187.

or as Bamberger says (*loc. cit.*), the elongation of the large arteries, as Kölliker has recently demonstrated by vivisections. But, in our case, we could not follow the direction of the heart from above downward, while we could follow it obliquely from left to right.

I have been unable to discover, in the case of deformity just described, any proof of the rotation of the heart on its transverse axis.

Such are the remarks suggested to me by the above case of deformity; I lay them before the profession, convinced that a more intelligent and experienced observer will add to their number and their value.—*Archives Générales de Médecine*, December, 1857, p. 709.

Note of a Case of Poisoning by Hydrochloric Acid, observed at Pondicherry. By Dr. Collas, Surgeon-in-Chief; Head of the Sanitary Service in the French Establishments of India.

Cases of poisoning by hydrochloric acid are rare. M. Devergie, who quotes but a single example of the kind, says so in these very words:—Orfila, who, in the fourth edition of his "Toxicologie," furnished only one instance (the same as that brought forward by M. Devergie) in the fifth edition of the book gives three other cases, none of which, with the exception of that borrowed by both the above authors from Dr. Serres, relates to a recent example of poisoning.

This species of poisoning is perhaps rarer in England than in France, at least if I am to judge by the laconism of the three lines devoted to it by Dr. Taylor in his "Medical Jurisprudence."

Although the Hindoo druggists are perfectly acquainted with the three principal mineral acids, and know how to prepare them, as these acids are employed only by certain operatives, their existence and their properties are unknown to the majority of the population; accordingly, in the interesting work published by Dr. Norman Chevers on the "Medical Jurisprudence of the Bengal Presidency," we find but a single case of poisoning by an inorganic acid—sulphuric acid.

The case of poisoning which this note refers to, probably the first of the kind observed in India, is attended with the greater clinical interest, inasmuch as, setting aside the rarity of this kind of accident, Orfila and the other toxicologists have given no special description of the symptoms of poisoning by hydrochloric acid: they have confined themselves to simply referring to the general indications of poisoning by caustic acids.

On the 5th of October, 1856, at the hour of my morning visit to the Maison de Santé, in crossing the entrance court in order to pass from the female to the male side, I perceived sitting on the ground, and supported by several tamijers, Sinivassin, of caste Vannia, aged about 28, by trade a packer; I was told that he had just swallowed a quantity of *néroupoutanir* (literally, fire-water), a name given in Tamul to aqua fortis. I desired M. Lépine, pharmacien of the third class, who attended my visit, to give him immediately

soap and water (common soap, one drachm and a half; water, six and a half ounces), while we sent for magnesia to the pharmacy, which is at some distance from the hospital. A few minutes afterwards, on arriving at the bed in which Sinivassin was laid, I observed the following symptoms:—

The patient was sitting up in bed; he could not lie on his back; he had no convulsive movements; his head was turned very much backward[a]; when questioned, he brought it forward again by a rather sudden motion; but as this position seemed to fatigue him much, he *threw* it rapidly back; the mouth, which was half open, exhaled no disagreeable odour[b]; the respiratory movements were frequent, and were accompanied by an isochronous moan; there was neither stain nor eschar on either the lips or the skin of the face. The gums were sound, but pale; the teeth presented no abnormal discoloration; the tongue was large, and had lost its colour[c]; at its centre, and a little to the left, was observed a furrow denuded of its epidermis, commencing close to the point, and ending near the root, at the bottom of which the papillæ of the organ stood up quite red[d]. The skin was cold; the pulse very small and very quick. The epigastric region was tender to the touch; no urine had passed since the evening before; there was no diarrhœa. The patient vomited neither the soap and water nor half a drachm of calcined magnesia given him by M. Lépine before I reached his bed.

On endeavouring to obtain some information as to how the poisoning occurred, and the quantity of poison ingested, I learned that those who accompanied the patient, yielding to the instinctive love of falsehood which constitutes a prominent feature of the Indian character, deceived me in stating that the poisoning had only just taken place; they also deceived me in asserting that twenty-four hours had elapsed since the occurrence. Finally, however, I ascertained that the evening before, Sinivassin left his work at three in the afternoon to go to a canteen, where he bought for six caches (about three farthings) fermented callou[e], and drank it; that being

[a] In dogs poisoned with hydrochloric acid, death is almost always preceded by very violent convulsive movements, especially in the muscles of the neck and spine. In certain cases these muscles are so strongly contracted, that the head is turned very much backward, and forms with the spine a curve, the concavity of which is very decided. (Orfila, "Toxicologie," Second Edition.)

[b] A very strong odour exhaled from his mouth at the time of his baptism, and afterwards observed on his death-bed. (Orfila, *in re* Benardel.)

[c] Tongue of a fiery red; lips blackish (Orfila, Dr. Serres' case). "The lips, the tongue, the palate of the child, so tender, so rosy some hours before, were coloured black and hardened (*in re* Benardel). The interior of the mouth and of the lips is red in poisoning with hydrochloric acid." (Orfila.)

[d] All the Hindoos, as well as many of the people of Asia, Africa, and even of the inhabitants of Europe, drink without the vessel touching their lips; in a doubtful case we should not lay too much stress on the absence of lesions, or of stains on the lips or gums.

[e] Callou is the fermented sap of the *Borassus flabelliformis* and of the *Cocos nucifera*. A quart is sold for six caches. My planton often got drunk on three caches'

intoxicated, he went home, where he drank, at seven o'clock in the evening, the half, about two ounces, of a liquid supposed to be brandy, contained in a bottle[a], found, either the day of the poisoning, or two days previously, in a hedge adjoining his house. Sinivassin was immediately seized with vomiting, and the vomited matters *effervesced* on the ground; he was brought successively to two former pharmaceutical pupils, one of whom, believing he was only drunk, immediately gave him castor-oil, which he threw up at once, mingled with matters apparently bloody; but soon perceiving that he had to deal with a case of poisoning, he had a search made for the bottle, which still contained a portion of the poison, and he believed that he had ascertained that the latter was nitric acid. I at first found it impossible to procure this bottle[b]; but as the pupil alluded to had served a long time in a civil laboratory, and had been employed in the Government Pharmacy, I believed, notwithstanding that I was surprised at not finding the characteristic discoloration on the tongue, that I had to treat a case of poisoning with nitric acid (*pottle ouppou travagam*, in the Tamul language), which is frequently employed by the natives, while they scarcely ever make use of hydrochloric acid (*ouppou travagam*, spirit of salt).

This was a mistake; for, subsequently commissioned, with M. de Nazeille, pharmacien of the second class, to investigate the nature of the fluid contained in the bottle which had enclosed the poison taken by Sinivassin, we easily proved, by means of the most characteristic chemical reactions, that this bottle, notwithstanding the care with which it had been drained, had held hydrochloric acid; but this mistake could not have been prejudicial to the patient, as the treatment of poisoning by mineral acids is the same for all.

It was therefore twelve hours since Sinivassin had swallowed about two ounces of hydrochloric acid; he had vomited copiously, and taken enough of soap-water and of magnesia to neutralize the effects of the small quantity of uncombined acid which could have remained, after this lapse of time, in the stomach; what was I to do? Referring to the general principles of poisoning by acids, Orfila, who sanctions energetic antiphlogistic treatment of "gastro-enteritis developed by acids," only when "the symptoms do not yet indicate ulceration of the digestive organs," advises the administration of mild mucilaginous drinks[c]. M. Bouchardat, attributing

worth; there are other persons who require to drink three or four quarts before they fall into the state of gross drunkenness produced by this beverage.

[a] This bottle was said to have been stolen from the house of a European who was getting work done in metals, either by Sinivassin or by some one else, who, profiting by his intoxication, made Sinivassin drink the acid, saying it was brandy; but as the verdict stated that Sinivassin had poisoned himself, there can be no more than a probability of the truth of this latter version.

[b] It was only after a domiciliary visit that it was obtained.

[c] "If several hours have elapsed since the occurrence of the poisoning, and if, in consequence of copious vomiting, or of considerable alvine evacuations, there is reason to believe that the acid has been totally expelled, the use of antidotes must be given up, and the soothing drinks, of which I have spoken, are to be administered."—*Traité de Toxicologie*, Fifth Edition, page 123.

death in cases of poisoning with acids to coagulation of the blood and consequent asphyxia, recommends a continuance of alkaline substances, which, useful at first as chemical neutralizers, are subsequently advantageous in virtue of their liquefying properties. The Italian school—generalizing a method which has been more than once successful when the poisoning has not given rise to *organic lesions*, applying it with that inflexibility of reasoning which astonishes and seduces beginners and enthusiasts, and makes them believe that the premises from which the deductions are so mathematically drawn must be well established; not taking into account what they study so closely in the natural history of the medicine—would have driven me to disregard disorders produced by the *chemical action* of the acid, and to attend only to the dynamic phenomena it produced; as if it were possible to deny these chemical phenomena, and the fatal reaction which fearful sufferings, and the partial or total disorganization of the gastric mucous membrane, have upon the entire system. As if it could be logical, or even possible, in other cases than those in which the ulcerated mucous membrane is henceforward incapable of reaction, to leave out of consideration that serious form of inflammation of the stomach, *toxical gastritis*, in order to attend to the dynamical symptoms, to combat a state of depression by means of exciting vinous and alcoholic drinks.

Setting, then, aside the school of Giacomini and Roquetta, I had to choose between the prescriptions of Orfila and those of M. Bouchardat; but, on examining closely, we find that the formula of treatment of the able professor of pharmacy is the result of a theory which is not without analogy to that of the Italian physicians. The latter, in fact, after having rudely reproached Orfila with having, in his treatment of the poisoned, attended only to local phenomena, after having closely studied and accurately classified the symptoms produced by medicines or by poisons, all on a sudden leave out of view the physico-chemical phenomena, otherwise called local, and make the treatment of poisoning consist in that of the dynamic or general symptoms. On the other hand, we cannot avoid reproving M. Bouchardat for omitting the local and vital phenomena, in order to attend solely to the secondary chemical action of the poison on the blood, an action which, incontestable after death, is perhaps not very certain when the living blood is concerned. Analogically, this doctrine would oblige us to conclude that alcohol and sulphuric acid have in reality but one and the same mode of action on the system, the coagulation of the blood, to which, in either case, we should oppose the effects of special alkaline substances, ammonia in the one, magnesia and bicarbonate of soda in the other; alcohol and mineral acids should therefore have, in the main, identical properties! By no other reasoning have the Italians arrived at their celebrated division of medicines and poisons. But these are not the only logical consequences of an exclusively chemical toxicology,—the imperishable axiom, "naturam morborum curationes ostendunt" compels us to believe that the venom of serpents, and that of insects

(*à priori* supposed to be identical, and recognised as always like those of the Ophidians), poison in the same manner as alcohol and the mineral acids[a]. In fact, what is prescribed in all these cases? Ammonia, which liquefies the blood.

However, the success obtained by M. Bouchardat at the Hôtel-Dieu is undoubted; and I have only to inquire whether cure is due to the liquefying property of the alkalies, "which follow the absorbed acid," and which, themselves absorbed, go "to dissolve the coagula in process of incipient formation." I confess that for my part I shall be as anxious during this second chemical neutralization of the poison (for it is nothing else), as those who fear, when administering bicarbonate of soda, seeing their patients who have been poisoned by an acid asphyxiated by the necessary disengagement of a great quantity of carbonic acid. In a word, how are we to admit that the sulphuric acid contained in the blood[b] shall not displace the acid of the carbonate, which we endeavour to place in excess in that fluid, and how shall we not tremble for the fatal consequences! But it is incontestable that M. Bouchardat has cured cases. Why? Because, by means of from five to thirteen drachms of pure hydrated magnesia, suspended in a quart of water, given previously to the administration of the bicarbonate of soda, to his patients, who had, as is almost always the case, already vomited, he had neutralized the acid, and perhaps also because he had to do with cases which were not very serious; for I doubt that a stomach, not ulcerated, but merely inflamed to such a degree as to produce toxical gastritis, could have borne these solutions of bicarbonate of soda (two and a half drachms to a quart of water), which he advises us to prescribe *abundantly*. Would it not, moreover, be possible that this bicarbonate, long since classed among alteratives, should act simply as an antiphlogistic, as M. Lemaire suggests in a work analyzed by M. Bouchardat (Annuaire for 1854).

The cessation of vomiting, and the tolerance of the stomach for fluid, appearing to me to be in Sinivassin's case indications of a profound disorganization of the mucous membrane, I ordered sweetened gum water, chicken-tea, and the sixth of a grain of watery extract of opium, the last to be given every hour in the form of a pill; a large opiate cataplasm was, in addition, to be applied over the entire abdomen.

About 2 o'clock in the afternoon there was a sensible improvement; this was of short duration. At 3 the patient passed about a pint of urine; at 5 he was in agony, sitting up in bed, his head being thrown strongly back, and resting on the top of the pillow which supported him; he raised it when I spoke to him, looked at me, and quickly threw it back again; at 7 he was dead.

[a] Bouchardat, "Nouveau Formulaire Magistrat," page 54; 1854.

[b] "M. Bouchardat, who has published this observation, thinks that the clot of blood found in the femoral artery, contained sulphuric acid, which was not in the saline state."—Orfila, *Médecine Légale*, Empoisonnement par l'Acide Sulfurique, Obs. ii., p. 131, Fifth Edition, Second Part.

The post-mortem examination was made thirteen hours afterwards, on the requisition of the Procureur Impérial, who associated with me M. de Nazeille, Pharmacien of the second class of the Navy.

Although during these thirteen hours the mean temperature was about 84° F., Sinivassin's body *did not exhale any odour*, nor did it present *any sign of putrefaction;* the cadaveric rigidity was extreme, and continued to the end of the examination.

All the organs, except the digestive tube, were in the normal state.

The gums and the arch of the palate were pale, without any alteration of tissue; the epidermis of the tongue, reduced to a grayish pap, was removable by the slightest scraping, leaving the papillary structure of the organ bare; the epithelium of the velum palati was in the same state; the œsophagus presented a very remarkable *condition of corrugation*, its longitudinal folds were prominent, and appeared to be formed by the juxtaposition in parallel linear rows of little reddish papillæ, *extremely dry, and very rough to the touch.* As to its general coloration, this tube was much paler in its middle than in the upper and lower thirds, where the tint inclined to black, though without any charring; it appeared as if the middle third had been a much shorter time in contact with the caustic fluid than the two other portions. This supposition became certainty when, on touching different points of the œsophagus with a solution of caustic potash, we saw manifest itself on these points a bright reddish-brown colour, more intense in proportion to the distance from the middle part, where it was rather weak, although very sensible; the tongue afforded the same reaction, which everywhere subsequently resisted repeated washings.

Viewed externally, the stomach was seen distended with gas; towards its lesser curvature, and on its inferior aspect, was a very extensive ecchymosis; everywhere else its peritoneal coat was marbled with red, green, and black; separated from the body, and opened between two ligatures, it gave vent to the fetid gases which distended it, and to upwards of six ounces of a dirty putrid fluid, of a dark yellow colour, and in which floated a black detritus. Throughout the entire extent of the organ, with the exception of a space of three or four inches from the pylorus, the epithelium of the mucous membrane had been completely charred; a part of the carbonaceous matter had become detached, leaving exposed the dermis of the mucous membrane, and was floating, as I have said, in the dirty mass contained in the stomach; what remained *in situ* was as black as charcoal, and had a pulpy appearance. The points whence the epithelium was detached were as white as old ivory, throwing into relief the black colour of those where it was still adherent. At two or three inches from the pylorus the intact mucous membrane was of a dark colour, resembling the lees of wine.

The paleness of the duodenum contrasted in the most decided manner with the lesions of the stomach; it was perfectly healthy, and, as well as the jejunum and ileum, contained a yellowish pap;

solution of potash did not produce any modification of colour in the mucous membrane of the duodenum. It is evident that the hydrochloric acid had combined with the alkaline principles of the bile.

The four cavities of the heart, the large arteries of the thorax, and the abdominal aorta and its branches, were filled with a red hard coagulum, which was persistent, and was perfectly moulded to the cavities containing it.

We delivered to the examining magistrate, who was present at the operation, according to the usual legal form, the œsophagus, the stomach, the fluid from the stomach, and about a pint of urine voided during life.

Although in a case of poisoning with hydrochloric acid, chemical analysis is of only secondary interest, on account of the presence of the normal chlorides of the system, and of those contained in the liquids we use, the researches we undertook, on the requisition of the Procureur Impérial, will, perhaps, be not wholly uninteresting:—

Urine voided during life.—Notwithstanding the strong and repulsive odour of ammonia exhaled by this fluid, it reddened litmus paper, and was copiously precipitated by nitrate of silver.

Fifty grammes of the urine, into which a solution of nitrate of silver was poured until it ceased to be thrown down, yielded a precipitate which, when properly washed and dried, weighed 1·707 grammes, giving of normal chlorides the enormous proportion of 34·14 in 1000 parts of urine, or 8·95 of hydrochloric acid.

The stomach and œsophagus were macerated for sixteen hours in distilled water; the liquid furnished by this operation was not acid; it was abundantly precipitated by nitrate of silver. One-fourth of this fluid, one-fourth of the stomach, and an equal portion of the œsophagus, were placed in a porcelain capsule; when the watery part had been evaporated, the mass was introduced into a glass retort, placed in an oil-bath, and some sulphuric acid was added; the gases and vapours were received in a solution of nitrate of silver, and quickly yielded abundant precipitates; the operation was stopped when the matters contained in the retort had become perfectly dry.

These precipitates, well washed, were treated during twenty minutes with boiling nitric acid; the residue, carefully washed and dried, weighed 70·912 grains; its physical and chemical properties proved it to be chloride of silver.

The gastric fluid was not acid; it had a repulsive odour; it was abundantly precipitated by nitrate of silver. The albumen was coagulated by heat, and separated by filtration.

One ounce and five drachms were distilled in a glass retort without the addition of sulphuric acid, yielding a product which was not precipitated by nitrate of silver.—*Annales de Hygiène Publique et de Médecine Légale*, January, 1858. P. 209.

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